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PHILADELPHIA FOUNDRYMEN'S ASSOCIATION.

The regular monthly meeting of the Foundrymen's Association was held at the Manufacturers' Club in Philadelphia on Wednesday, October 6, the president, P. D. Wanner, occupying the chair. There was a fairly large attendance of members.

The report of the Executive Committee for the month just passed showed little or no change in the condition of the foundry trade.

THE DEFENSE ASSOCIATION.

W. H. Pfahler, of the Abram Cox Stove Company, Philadelphia, chairman of the Committee on Defense appointed at the convention of the National Foundrymen's Association recently held at Detroit, by invitation then addressed the gathering in regard to the progress made by his committee. He said: "Acting under the instructions of the convention the committee appointed to canvass the matter have been in correspondence with a large number of manufacturers, nearly all of whom they found evinced a desire to take up the subject and work out the problem of defense. There seems to be no doubt in the minds of those we have corresponded with as to the advisability, and little doubt as to the practicability of the formation of a defense association. I called a meeting of the committee here about two months ago, and after

a two days' session we believe we reached a line of formation which will be acceptable to the foundrymen. We at once commenced correspondence with about 200 of the leading foundrymen of the country, selected at random, asking them if they were sufficiently interested in the matter to attend a meeting for the discussion of the subject. Although that invitation to a discussion has only been out about ten days, we have already over 100 acceptances. I have, therefore, called a meeting, to be held in Cincinnati at the time of the meeting of the Western Foundrymen's Association the week after next, which will get those founders located west of Ohio to attend, and in November we expect to have a meeting in the east. Between those two meetings we have little doubt but what we can formulate a plan which will be acceptable. It is the purpose of the committee after those two meetings to have a general meeting for the purpose of adopting by-laws and putting the association in working shape. I found in New England, especially, where there was formerly a great aversion to anything of the kind, the strongest advocates to-day in favor of an organization which will be counter to the organization of the employes, and the idea that such an organization as the former must be antagonistic is dying out, because to-day the warmest advocates in its favor are to be found among the laboring men themselves. They realize that in an organization such as contemplated they will have a strength against agitation and the agitators who are in the minority always in the labor columns. There is not 20 per cent of the Molders' Union to-day who are in favor of a strike, and yet that small percentage could bring about a strike. With the manufacturers organized they will have a support which is always maintained. The committee are very much encouraged with the work that has gone on so far, and I hope after the meeting in Cincinnati to be able to report a great deal of progress."

W. C. Henderson then read the following paper on

"THE CONVERSION OF WHITE IRON CASTINGS INTO STEEL."

The production of steel castings, the result of molding liquid steel, is fraught with difficulties of a persistently dominant nature.

For instance, green sand may not be used for a mold into which it is intended to introduce melted steel, because the water in the sand chills the metal at the gates and so prevents its running. No better success will be obtained by employing a dry mold of ordinary sand, because the fluid steel is at a greater temperature than the fusing point of the mold. With a dry sand mold, faced with a substance requiring a greater heat to become melted than that possessed by the entering metal, some success has been obtained, though by no means entirely satisfactory, even in casting very large thick shapes.

Steel is only held fluid with great difficulty, owing to its excessively high melting point, and upon entering a mold several thousand degrees cooler the metal immediately in contact with the walls of the mold becomes solid and fixed beyond the possibility of yielding to the subsequent contraction which steadily takes place as the mass cools. Unless a liquid connection is maintained between the center of the casting and the source of supply until the metal has become solid throughout, the casting will be defective through internal shrinkage.

Supposing all of these details to have been carefully worked out and provided for, there still remains the most serious difficulty of them all—namely, the prevention of the occlusion of gases, which convert the result of the operation into a molded steel sponge.

An exceedingly attractive and seemingly hopeful procedure toward the surmounting of these obstacles lies in the converse efforts to bring the metal conditions to suit the mold, instead of striving to wholly adapt the mold to the metal. The "Terre-noire" and the "Mitis" processes may be taken as illustrative of what has been attempted in this direction. It was hoped (and not entirely in vain) that the addition of silicon, in the one instance, and aluminum, in the other, would effectually prevent the occlusion of gases, and at the same time so lower the fusing temperature of the steel that it could be readily molded.

Where it is desired, however, to obtain castings of intricate shapes and small dimensions so much has been left desired in the application of these direct methods that in order to meet the

demands for castings of this class considerable metallurgical license has been indulged in, in the more or less successful operation of purifying and converting castings, primarily made of white pig iron, into steel, by first extracting all of the carbon, as in the case of malleable iron, and next introducing an amount of carbon by a cementation process.

Small castings, by courtesy called steel, are at present almost entirely made by this process, and are giving fair satisfaction in some cases. Some of the manifest drawbacks to this method are the length of time necessary for the operation (requiring a period of not less than three weeks), the inability to control the amount of carbon introduced during the second part of the process, and, finally, the total failure to have removed from the castings any of the sulphur, phosphorus, silicon, manganese, arsenic, etc., which were, of course, present in comparatively large proportions in the white pig iron from which the castings were originally made.

The process operated by the New England Steel Casting Company, Philadelphia, occupies a place, as nearly as I am in a position to judge, not knowing of what the process consists—it being a secret one—just between the white iron method as now practiced and the doctored steel. Castings are made in green sand, and have the appearance of being white iron. They are submitted to a converting process, which extends over a period of from two to six days (dependent upon the size of the castings treated). It is claimed by the owners of this process that the castings produced by them are fairly free from the objectionable metalloids; have combined carbon in any desired percentage no graphite carbon whatever); have a tensile strength of 70,000 pounds and upward, according to the carbon content; will bend and draw out cold after the manner of wrought iron; will temper, weld, and take a high polish.

DISCUSSION.

Mr. Evans: Is it possible to make castings of any size larger than the samples we have here before us? I hardly suppose it would be possible to make cylinders for locomotives or bed

plates for engines. There must be a limit somewhere. What is the limit?

Mr. Henderson: I believe the company propose to make a specialty of small castings, taking them up where the other processes find it difficult to secure satisfactory results. Where the Mitis process can be used it is, I believe, cheaper than this process.

Mr. Devlin: Do you know whether it is possible to make a casting of that character any more solid than you would a casting of the same character in malleable iron?

Mr. Henderson: I believe it is claimed that they do that; but I know of no reason why malleable iron could not be made as solid.

Mr. Flagg: Mr. Henderson said the castings required a treatment covering in some cases two days, in others as long as six days. How would it be arranged that one casting should get two days and another six days? Would they classify the castings or have separate ovens?

Mr. Henderson: I believe they contemplate doing both. They classify the castings at present. They intend to have a soaking pit in connection with the process, and take castings out during any stage of the operation.

Mr. Rominger: What would be the expense compared with malleable iron?

Mr. Henderson: It would be cheaper.

Mr. Outerbridge: Have you made an analysis?

Mr. Henderson: Incompletely only.

Mr. Outerbridge: Of course, I know you are familiar with quite a number of processes for converting iron into steel. Some of them have been before this Association. We have seen axes and various small castings precisely similar made with white iron originally. All these processes start necessarily with white iron, which is about the same specific gravity as steel. It is conceivable that there might be a process for oxidizing out the excess of combined carbon in practically a small section. We remember

that a Mr. Bates some years ago had a process advertised quite extensively, and many people invested large sums of money, went in on the ground floor and came out in the attic. I do not mean to intimate that there is any relation between that process and the one before us. A year or two ago Dr. Hunted was before us and exhibited a number of castings similarly treated; and I believe a modification of his process is in use in a foundry in Frankfort, Pennsylvania, to-day. I should like to ask Mr. Henderson whether this process is a new process, or whether it is analogous to any of the other processes with which he is familiar, and whether the result is superior and cheaper.

Mr. Henderson: I have been called upon professionally to investigate most all of these processes. I have not been able to investigate this process fully; but in such experiments as I have made I have found no carbon at all. There is no graphitic carbon at all.

Mr. Devlin: As I understand it, you have only as yet examined small, thin castings. From your experience in analyzing thin malleable castings have you found them to contain graphitic and combined carbon, or not?

Mr. Henderson: In some instances I have found considerable graphitic carbon. In fact, some malleable iron people told me, when on a visit to Chicago last week, that it would be impossible to have malleable iron castings without graphitic carbon. Some castings I know to be entirely free from graphitic carbon, and also practically free from any trace of combined carbon.

Mr. Outerbridge: Does not that depend upon whether they start with pure white iron?

Mr. Henderson: There is one point I picked up in Chicago that struck me as rather singular. It is the custom there to use brickdust, ashes, sand, or anything of that nature, and they really effect the change from the combined to the graphitic state. As regards the malleable iron, all I saw contained only 2 per cent graphitic carbon; but they make no attempt to eliminate the carbon.

Mr. Evans: Are the owners of the process ready to take orders commercially?

Mr. Henderson: Yes; they have a plant in operation; it is not very large at present; but they are doing the work.

Mr. Outerbridge: How large a section can they convert?

Mr. Henderson: I have not seen anything over two inches.

PROCEEDINGS OF THE WESTERN FOUNDRYMEN'S ASSOCIATION.

MONDAY MORNING SESSION.

The fall meeting of the Western Foundrymen's Association was held in Cincinnati October 18, 19 and 20, 1897, at the Grand Hotel. The opening session was called to order at 10 a. m. Monday, October 18, by the president, C. A. Sercomb. Mr. George McG. Morris, chairman of the local committee of arrangements, introduced the Hon. Jacob H. Bramwell, member of Congress for the second Ohio district, who made an address of welcome in a happy vein, introducing some of the foundry vernacular. President Sercomb responded for the association, telling some of the characteristics of the foundrymen who had come among them for a few days, and commenting on the vast advance that had been made in foundry practice in recent years. Invitations were then read by the secretary from various manufacturing institutions to the foundrymen to visit their plants, among them being invitations from the S. Obermayer Co., John B. Morris Foundry Co., J. A. Fay & Egan Co., Davis & Egan Machine Tool Co., Cincinnati Milling Machine Co., and others, and also from W. McCallister, president of the Chamber of Commerce. Upon motion of George M. Sargent a note of thanks was tendered to the people extending the invitations, and they were accepted.

There being no objection, the reading of the minutes of last meeting was dispensed with, as the same had been published.

The secretary stated that the Editing Committee, who were instructed to publish the by-laws as amended, had failed to do so, but had presented a report to the effect that the by-laws had been prepared for publication and asking for further instructions in the matter. There being no objection, the chair stated that further action would be deferred until the next meeting of the Association.

On motion of W. N. Moore the secretary was instructed to obtain from the various foundries in the territory covered by the

Association as much information as is possible regarding their present business, and their regular capacity, and to tabulate this information for the benefit of the members of the Association. The secretary was also instructed to obtain this information periodically hereafter. An amendment carried, providing for a committee to start the inquiry as to an improved condition of the foundry business, among foundrymen in attendance at this meeting, this information to be extended later by letter inquiry from the secretary's office. The committee was made to consist of W. N. Moore, of Joliet, Ill.; F. M. Lyon, of Aurora, Ill., and Geo. M. Sargent, of Chicago.

Wm. H. Pfahler, of the Abram Cox Stove Co., Philadelphia, was asked to address the meeting. He spoke of the extension of American trade in foreign markets as the greatest desideratum of the manufacturer to-day. Inquiries from foreign parts are steadily increasing and the efforts of the National Association of Manufacturers are being rewarded with gratifying success. As to the sharp competition which foundrymen find in all parts of the country, he considered that the chief cause of demoralization was the effort of manufacturers in one district to unload surplus product in distant fields, and the retaliatory policy of those whose territory they invaded. He thought the tendency of foundrymen's meetings was to educate foundrymen to cultivate their near-by markets, to inform them more perfectly as to the state of trade and to prevent the demoralization that came from the predatory policy referred to.

Martin A. Fox, president of the Iron Molders' Union of North America, was called upon. He said that while he might be regarded by some as an interloper, his visit was in the interest of all concerned in any way in foundry operations. He urged friendly relations between foundry employers and employees.

C. S. Bell, of Hillsboro, O., a vice-president of the American Foundrymen's Association, commended to Cincinnati foundrymen the benefits of organization.

The meeting then adjourned until 2:30 p. m.

During the intermission the Association repaired in a body to the Cincinnati Chamber of Commerce, where they received the privileges of the floor, and after a general introduction were formally welcomed by William McCallister, president of the Cincinnati Chamber of Commerce, in a brief speech in which he stated that the City of Cincinnati is especially interested in having various organizations convene in that city, and that he wished to assure the Western Foundrymen's Association that the citizens of Cincinnati would receive them hospitably.

President C. A. Sercomb, in behalf of the Western Foundrymen's Association, responded briefly. He returned the thanks of the Association for the courtesies of the Chamber and referred in glowing terms to the Hon. William McKinley, President of the United States, as one of the great men of Ohio which that State had given to the Nation, whose administration has started anew the wheels of industry, and promises to usher in an unexampled era of prosperity. Mr. Sercomb assured the business men of Cincinnati that their guests were having the best kind of a time.

MONDAY AFTERNOON SESSION.

The meeting was called to order at 2:30 p. m., President Sercomb in the chair. The following paper was then read:

"THE VALUE OF PHYSICAL TESTS."

By S. S. KNIGHT.

The advent of various systems for the grading and mixing of irons by physical tests has led me to think that this topic, although old, may yet afford opportunity for investigation. Especially within the last year has much antagonism been shown between the exponents of these systems of physical testing and consequent foundry regulation and those who are using the means of chemical science to the same end. It is a strange, but nevertheless noteworthy, fact that the promulgators of mechanical testing have but one criticism to make on the work of their antagonists, and that is that their employment is not

economical. As to how far their claims are substantiated by facts, we will now proceed to investigate.

According to the best authorities mechanical analysis is founded upon the discovery that the shrinkage in iron varies inversely as the silicon contained therein. Tersely speaking, the measure of shrinkage is equivalent to the determination of the silicon. To experimentally determine if such be the case, two bars were cast one foot long and one-half inch square, the same being the measurements preferably used by mechanical analysts. Both bars were cast from the same ladle of iron, one with the metal very hot and the other after the metal had been reheated by the addition of less than five-tenths of one per cent of ferro-manganese. Time was given for the surface of the metal to strongly glow and then the second bar was poured. The bars analyzed as follows:

FIRST BAR.		SECOND BAR.	
Silicon	2.256 per cent.	Silicon	2.197 per cent.
Phosphorus650 " "	Phosphorus648 " "
Manganese712 " "	Manganese	1.112 " "
Shrinkage per linear foot..	.11 in.	Shrinkage per linear foot..	.17 in.

Both bars were cast in the same position, and aside from the treatment mentioned all conditions were held as nearly alike as possible. According to theory our proportion of silicon in the second bar should be 11:17::X (or silicon in the second bar) :2.256 or X, equals 1.459. You will note that this is .738 per cent less than the actual silicon contained. We are not surprised that Mr. Keep and the other exponents of this system are more or less prejudiced against chemists at large if they suppose that one-half of one per cent is the ultimate accuracy of their practical knowledge.

Let us look at the same case from the practical side. Supposing we knew the silicon of the first and second bar and the shrinkage of the first, to find the shrinkage of the second. 2.256: 2.197::X (or the shrinkage of the second bar) : 11 (the shrinkage of the first) or X, equals .15.

Suppose this iron were being used in a 24-foot cylinder flanged at both ends, then the error in estimating the shrinkage

would almost be one-half of an inch, or enough to insure both flanges shrinking off. And yet in the face of all this it is claimed that the physical test system is economical. Let us take a glance at how much saving would result from the use of this system in a shop casting one such cylinder in a flask every day. From our previous observation we see that if the rule were adhered to our casting would be lost regularly, and counting \$10 as the total cost other than metal for such cylinders, we would have in one year of 310 working days \$3,100 lost on the work alone, or enough to maintain a chemist and laboratory in opulence.

No doubt Mr. Keep will say that if the first casting is lost through shrinkage, any one would use more soft iron. In that case his system becomes but a means of guessing and not an accurate, scientific method. When capitalists' millions and all of the employment resulting therefrom are submitted to the chance of a guess, no theory that maintains such a performance can be called a discovery or its constant use be honored by the name of a system.

When it is stated that it was discovered that shrinkage varied inversely with the quantity of silicon contained in the metal, it should be known that the discovery was only partly made. The facts are that shrinkage varies directly as the content of sulphur, manganese, combined carbon and titanium, and inversely as the silicon and phosphorus content. However, this variation is in no case in a true ratio, since the more of the affecting metalloid the iron contains, the more is its action facilitated or hindered as the case may be.

We suppose that all will acknowledge that each foundryman should strive in every way to better his profession. If it were not for this constant groping after the truth and the actual facts, what would cause the large attendance at the meetings of the associations? In connection with this desire for facts, please think of two foundrymen, one possessing a laboratory and chemical facilities and the other with a testing machine, price \$200. The first through constant experiment and careful analysis would soon become a master of his trade, while the other would

be bound completely by the rules of the system he was using. He would be forced to bid only for work such as he had made, since he would be unable to form any idea of the requirements or cost of new work. While the first could be constantly cheapening his process and adding to his store of metallurgical knowledge, the second could never make any discoveries or advancements in mixing his metals. Let me give an example taken from fact of the actual utility of chemical facilities. A foundry never before having made alkali kettles wished to bid upon such work. A sample of an old kettle was obtained and analyzed. The work was secured and 15 ten-ton kettles made without a single loss. Such facts speak for themselves and show conclusively the actual value of scientific methods as applied to practical industries.

Of all the discoveries that have been made in recent years in iron metallurgical work there has been only one that a foundryman equipped with a mechanical testing outfit could possibly have made and it is a noticeable fact that even this was made by a chemist. I refer to Mr. Outerbridge's discovery; and here it would be well to observe that facts are found out and discoveries made only after patient, methodical, intelligent toil, and are very seldom, if ever, the results of luck, guesses or haphazard work. Doubtless had it not been for his chemical research along the line of his discovery Mr. Outerbridge never would have arrived at the conclusions which have made him so justly famous.

Before leaving this phase of our subject, let me cite another instance where chemical science scored a point that mechanical testing never could have made. While using high manganese irons we found that our test-bars made from these mixtures invariably broke, higher than a corresponding mixture of low manganese metal. Scores of analyses were made to prove the cause of this phenomenon and in every case high manganese was shown with no other phenomenal feature. Low manganese irons were tried having enough ferro-manganese added in the ladle to place the content of this element over one per cent. The results were the same as if originally high manganese iron had

been used. The two test bars given below will show for themselves the advantage of gain in ultimate strength to be made in this manner. Both bars were cast on end and broken 36 inches between supports on a Riehle machine having the load applied at the center with a uniform acceleration.

First bar:

928 pounds breaking strain.
88.100 of an inch deflection.
.971 inch thickness.
.13 inch to one foot shrinkage.
.950 inch width.

Its analysis was as follows:

Silicon	1.98 per cent.	Sulphur050 per cent.
Phosphorus558 " "	Manganese	1.002 " "

This bar had a small amount of ferro-manganese added in the ladle, the iron being held until it flashed strongly before pouring.

Second bar:

736 pounds breaking strain.
.97 inch deflection.
.988 inch thickness.
.12 inch to one foot shrinkage.
.990 inch width.

The analysis of this bar was practically the same as of the first except in manganese, which was .712 per cent. It was also a plainly noticeable fact that the chill was less deep and the grain more uniformly close. As far as known this work was original with the author, and although the gain in strength and diminished chill was so great no detrimental qualities were given the iron except the increased shrinkage, which was directly due to the increase in combined carbon, which the metal took from the ferro-manganese.

Hundreds of corroboratory bars and other tests might be given, but these will prove the correctness of the statements made as well as any evidence could. In connection with manganese and its influences upon chill it will no doubt be interesting to know that many car-wheel foundries are using ferro-manganese

to keep down the chill, and thus are at liberty to use greatly increased quantities of remelt in their mixture and very little pig metal. The ferro-manganese is added in the ladle as described and as much remelt carried as 85 per cent. The action of the manganese seems to be to increase the combined carbon and to eliminate the sulphur. The first effect will be readily explained by the analysis of ferro-manganese, which will run about as follows:

Manganese	80 per cent.
Combined carbon.....	8 " "
Iron	12 " "

The second or eliminating action has as yet not been satisfactorily explained, although it is known that preferentially sulphur combines with manganese instead of iron. It is known, however, that the action is greatly facilitated by the presence of a basic slag and all metallurgists who have investigated the science of metallography agree that no pig metal even when cold is free from slag, so that when liquid, its presence is almost certain. Presuming this to be the case, the reaction would take place as in the basic steel furnace; the sulphur of the iron sulphide combining with the manganese and again combining with whatever base of the alkalies or alkaline earths that might be present. In cooling, this slag would naturally separate itself from the metal in a larger degree, and hence its method of eliminating sulphur.

While speaking of the relative value of physical tests, it may be interesting to consider briefly their worth as absolute measurements of the qualities of various materials. It will be understood that the value of any test is in a direct ratio to the similarity of composition and structure existing between the test piece and the finished product whose qualities are sought. Since Bessemer steel is more homogeneous than most other grades, tests taken from it are far more correct and more closely approximate the qualities of the heat than those taken from open-hearth billets. However, in the best Bessemer ingots the segregation of phosphorus is so great that tests taken before the metal has

passed through the mill are of little absolute value. How much greater than must be the chance of error in determining the qualities of cast iron which has been melted in a cupola, where the more fusible components of the iron mixture melt and are tapped out, leaving the more refractory to come out at some future tap.

It has been common experience in one of the large foundries that the first tap of the day was uniformly softer than that of any other time during the heat, when the same mixture was being charged on. A few analyses upon this point taken on three successive days may be of interest.

FIRST METAL TAPPED OUT.		METAL FROM FOURTH TAP.	
Silicon	2.739 per cent.	Silicon	2.102 per cent.
Silicon	2.891 " "	Silicon	2.007 " "
Silicon	2.624 " "	Silicon	1.992 " "

In connection with what has been written of late upon the comparative fusibility of hard and soft metals, these analyses seem to bear valuable testimony. The author's intimation in a previous paper that invariably hard metals would fuse at a higher temperature than soft ones has been severely criticised. Lack of time has been the only cause for not making a number of experiments upon all brands and grades of metal to establish the truth in regard to this matter. However, the analyses given would seem to strongly support the author's theory, and it has been without exception our experience that regardless of the combination of metalloids, silicon is the most potent factor in determining the temperature at which cast iron melts.

Before concluding, one means of physical testing which has demonstrated its merit must be mentioned, and that is the microscope. The invaluable work which has been done in the last year by the foremost investigators in the field of iron metallography has made the microscope in the hands of the experienced and competent metallurgist, a necessary adjunct to every progressive steel mill and iron foundry. The discovery and isolation of martensite has cleared away the mists of ignorance and doubt concerning the cause of the famous

phenomenon of Gore. It only remains for the chemist to settle forever the dispute of the eminent microscopists, Osmond and Howe, as to whether this martensite is an allotropic form of ferritic iron, or but a kind of iron carbide, and if so to determine its composition. Personally it may be said that we have been using a microscope in our laboratory for the last year and it has given perfect satisfaction. The longer it is used the more necessary does it become and the more valuable does it prove itself to be.

The elaboration of the knowledge of the world upon the constituents of iron and its consequent physical properties has proved to be of great commercial value. For instance, in determining what iron to use in dynamo field castings, the amount of ferrite contained in the iron proves to be in proportion to the number of lines of forces per unit area that the metal will carry. Some pearlyte and cementite should be present, however, in order that the fields may possess the proper amount of residual magnetism. This shows one application of technical scientific knowledge to industrial work.

In conclusion it may be stated that the commercial value of a laboratory will be in direct proportion to the liberality of its equipment and to the competency of the operators employed. Practical tests will be found valuable only as they are made on reliable apparatus, and it may be put down for a fact that no system of testing or any machine will ever be equal to the ingenuity of a trained and competent man, situated with such facilities at his disposal that he may use his knowledge to the best advantage.

The secretary then read the following discussion of Mr. Knight's paper from Dr. R. G. Moldenke, metallurgist of McConway & Torley:

In that portion of Mr. Knight's excellent paper on "The Value of Physical Tests" which relates to the comparative fusibility of hard and soft metals, he describes an occurrence that may be seen daily in every foundry, namely, that the first iron melted of the charge is softer than the balance. Now, he claims

that this is due to the softer portion of the charge coming down first. How would Mr. Knight then explain the fact that the very same thing happens when the charge consists of but one grade of iron running uniform in silicon? Is it not because the first part of the charge in melting collects where it is no longer subject to the blast, in a comparatively short time, while the balance coming down slower is subject to oxidation to a continually increasing extent? In other words, with a uniform charge, aside from the first few ladles, which are always uncertain and generally hard, you could begin with soft iron, and wind up with material fit only for sash weights. I am among those who have criticized Mr. Knight in this matter, but I assure him with the best of motives, for we all thank him for bringing to light his first observation, for the valuable information which was brought out when the subject came to be studied. Mr. Knight, by his praiseworthy and conscientious effort to record facts as he sees them, can but gain in the estimation of those who beg to differ from his conclusions. Let us state the facts as we find them, and let the practical foundryman who may not agree with us interpret them in the light of his experience and to his best advantage.

Thos. D. West: The first part of Mr. Knight's valuable paper that I would comment upon is the statement he makes that "according to the best authorities mechanical analysis is founded upon the discovery that the shrinkage in iron varies inversely as the silicon contained therein." About a year and a half ago I read a paper before the Western Foundrymen's Association touching on that subject, in answer to a statement made by Mr. Keep to the effect that the contraction of a bar would define the exact silicon in the iron. I showed that we could make a combination of the metalloids by means of sulphur, manganese and phosphorus, that would vary this contraction, so that it is no guide to go by at all. It is impossible to define the exact silicon contents of any iron by simply showing the contraction of a test bar. Any founder by making a variation in the combination of his metalloids, may cause contraction that would practically be

the same and if he would send the results to Mr. Keep or anyone else that makes the claim that he does, he would find himself wholly wrong in attempting to tell what the silicon contents were. The second part of Mr. Knight's paper, which opens up some further fields for research, is the matter of his claim that the first tappings gave a higher silicon; and he goes on to say in consequence, softer iron. He don't tell us by what means he obtained the knowledge of whether it is really softer iron or not. The practical foundrymen will all agree with me that as a rule they are pretty scared of the first ladle that comes out of the cupola. I think the general experience of all founders is that the first tappings of the cupola they will pour into sash weights or something of the sort, in which they are not particular about the kind of iron to be used. The conclusions that Mr. Knight has come to are rather, as I might term it, "rattling" on that point. They don't agree with experience. It is to be regretted that Mr. Knight has not given us a thorough analysis of the iron. We might have discovered in it higher sulphur, an increased percentage of manganese, or something that would have been a hardening element. I believe his claim that soft iron will melt easier than hard iron is merely supposititious.

Guy R. Johnson: While I run only a small foundry, my experiments show that Mr. Knight is correct in every respect. To give you an example in point, I may say that in making chilled rolls out of melted iron in a cupola—it is a very poor way to make chilled rolls, but we had no better—we had to abandon altogether the use of the first iron because we found it too soft. We analyze all of our iron before putting it in the cupola, and take careful analysis afterwards. We find they lose much less silicon from the first heat than the average of the whole heat. We also lost sulphur. We use Pocohontas coke, which runs about as low in sulphur as the average coke. If we put that same iron in at the end of the heat, the sulphur went up slightly. We lost a little more silicon and we could get our chill to a dot. We were running a one-inch chill, and at the first heat got three-quarter of an inch for four days; then at the end we got an inch to

an inch and an eighth for six successive days. I think that bears out Mr. Knight's remark to a large extent. Another thing, some of us who have been arguing on the question of shrinkage, etc., have got a great deal to thank Mr. Knight for in his researches in that line. The idea that silicon is the only metalloid that really makes any great difference is all nonsense. Really, silicon has much less effect on the result and casting than sulphur, taken proportionately; that is to say, a casting that comes out .05 in sulphur will be a good open casting, and one with .07 to .08 will be very hard; yet the silicon in those castings generally varies less than 1 per cent, if you are using a mixture that you know the contents of beforehand.

Thos. D. West: The longer we hold iron in the cupola—or the longer it is in melting—the more silicon we lose; and that, no doubt, would have a bearing in the findings Mr. Knight has given us. For all that, it doesn't alter the fact of our finding the first ladle harder. It may be true, which I agree is a matter of experience, that Mr. Johnson has got a softer iron from the chilled iron. It has been only where we want to get a softer iron first that we found that we had the first melting hard, and as I stated before, probably if Mr. Knight had given us a complete analysis of his first meltings, we might have discovered something in them—either that the sulphur was high, or that the iron contained some metalloid in the latter meltings which would have had a hardening effect.

S. S. Knight: In reply to Mr. West as to the analysis of the iron we are using, it would only be fair to state that we very seldom melt iron which would carry over .04 in sulphur; consequently, the addition of sulphur to our iron in the first meltings in the cupola would hardly be possible. Our irons run between .035 and .04.

Mr. West: Sulphur is a very important factor. It takes less of it to affect iron than any other metalloid. I have found that out by experience.

D. B. Meacham: We never melted any pig iron, but several years ago instituted a series of tests, merely breaking tests. We

sent circulars around to the founders, asking them to make three bars, one at the first, one at the middle and one at the last heat. We found in taking these, not knowing what they used in their mixture, that the bar which came off from the first heat broke lower, and the last generally was highest; and the supposition was that the last bar from the under-heat was the harder bar. We knew nothing about the softness, because we didn't analyze the silicon contents, but merely obtained the transverse strength.

Guy R. Johnson: I should like to ask Mr. West what coke he uses, and how the sulphur runs?

Mr. West: We are using Washington coke. It runs about .80 per cent., and from that up to .85.

Mr. Johnson: And it is rather irregular, is it not?

Mr. West: No; in fact, we are pretty particular in sulphur and have got to be pretty strict with our coke people. It comes pretty regular, although it may go up to .90.

Mr. Johnson: How much of a bed do you use?

Mr. West: We run from 60 to 70 tons. We have a certain height and we bed the coke in upon measurement of height, according to the quality of the coke. In weight the bed is about 1,300 pounds. Then we have a certain amount that we put in between the charges that we measure out.

Mr. Johnson: Don't you think that that explains the hard ladle-full of iron? The first iron that comes out there is melted by the very large bed you have in. You have more coke in the bed than you put in afterwards—certainly every foundry has. We are using .08 and .09 sulphur. The first iron comes in and absorbs most of the sulphur from that coke. That accounts for the hard iron, whereas if you were using coke with only .6 and .7 per cent of sulphur you would not find it so.

Mr. West: Where do you get coke as low as that?

Mr. Johnson: In southwest Virginia. I have analysis made frequently of Pocohontas cokes and others. It rarely runs over .6.

Mr. Sorge: What is the size of your cupola, Mr. West?

Mr. Knight: I would like also to know the capacity in tons per hour.

Mr. West: The inside diameter of our cupola is 66 inches, and we melt about an average of from 60 to 70 tons daily. We run it down in about four and a half or five hours. We can often melt it faster than we can take care of the iron. We use a center tuyere. Our consumption of fuel varies; depending somewhat upon the quality. But we are melting all the way from one to ten up to one to fourteen. We have got to get a very solid good coke to get one to fourteen. We use all pig iron with "shop scrap"; if we had light scrap in mixture with the pig the iron would come down faster, and we would have to use less fuel.

W. N. Moore: Our business is the making of light castings, in which strength is not an important consideration, the shrinkage alone being about the only physical qualification with which we have to deal—shrinkage and softness. Our experience shows us that there are certain limits to which we can go, with reference to silicon and sulphur, particularly, and inside of these limits we are very safe. But when we get outside of them we fall down. We depend entirely upon the laboratory. And I think anyone who will take up the matter of applying chemistry to the foundry, and follow it up for a year, say, will never feel safe without knowing afterwards just what he is doing—how much silicon it takes to make a good casting of such and such a kind, or how little he can get along with; how much sulphur he can stand without liability to trouble, and also the relation of the carbon to his requirements. I don't think there is any modern foundryman who cannot study that with great benefit. In regard to the question of the first iron being hard—as it appears the discussion has centered upon that somewhat—it seems to me from the observation which I have been making, taking the results as I find them, that there have been local causes that produced these results. It has been my observation that the first iron from the cupola was hard. Whether the silicon was higher or lower I have never investigated; but I supposed there were hardly two

opinions on the subject. The experiments which are given here we can't question, and it would seem as though there must be some local conditions out of the ordinary experience to account for the results.

Henry H. Coe: My observation has been, so far as I can give it practically, that the first run of iron was a little more uncertain.

Mr. C. S. Bell: It has always been a fixed idea in my mind that the first iron was uncertain, and that it was not safe to put in castings that required any particular strength, or any particular softness; and, therefore, we used it in such castings as didn't require any of those special qualities. Now, there may be many reasons for this. In the first place, it depends largely upon the depth of the bed of coke, and the character of the coke in that bed. In the next place, it depends largely upon how you fire your cupola, how much charcoal you have in the bottom and how much you blow through the tap-hole before you close it up. We have never in all my experience sought to use the first iron as reliable iron. The first tapping may be sometimes softer than any other parts of the heat. I would account for that in two ways; first, there may be more charcoal in the bottom of the cupola; next, the iron that came to the melting point first may be a better quality. I suppose that you mean when you say first tap, not the first 1,000 pounds, but the first 100 or 150 pounds. So it is largely due to the character of the metal that was melted. If we are going to make a casting out of the first tap, we would not undertake to use the first iron unless the casting weighs from 1,000 to 1,500 pounds, when we would feel reasonably safe in using it. As to the question, which iron is easiest melted—that is, that which requires the highest temperature—hard iron, white iron, as we term it, there is a vast difference between the melting of different qualities of white iron. We probably melt more white iron than any other firm in the United States and we know that it requires a higher temperature to melt white iron. Probably if you were to measure the units of heat it would be accounted for in a different way from what is ordin-

arily understood. For instance, you want to make certain castings and it may require what we would call a very hot iron to run it; but on account of the short life of the white iron you have got to have a higher temperature than you would with gray iron. You might lose the temperature after you had it running freely; therefore, you would say it required a higher heat to melt. The fact is, it simply loses its heat more quickly than does the gray iron. But there is no doubt about the fact that it requires a higher temperature and more fuel to melt a heat of white iron than it does of gray iron.

B. Hoffman: We find in our experience that the first iron runs always a little harder than the middle iron, and the cause of that I attribute to the bed.

E. K. Rundle: I have always noticed in melting our iron that there has been an uncertainty about the first run. We cannot always depend upon it. I have always found though that the iron is apt to be a little hard.

George McG. Morris: We make light and medium weight castings, and when it comes to the first part of the heat—I have reference to the first ladles of iron taken out of the cupola—we never use it in our good castings; always pour it into some chunk we don't care much about. In important castings that we have to make we always take the iron from the middle of the heat. But, as I say, we have always regarded, probably as a result of practical experience, that the first of the iron was not good and we don't want to use it; the first ladle we use as a drier for the ladles, and then use it in chunks for unimportant castings; but never go as far as the whole of the first charge.

Mr. Moore: There is one paragraph in the paper which interests me very much: "The facts are that shrinkage varies directly as the content of sulphur, manganese, combined carbon and titanium, and inversely as the silicon and phosphorus contents." I note that free carbon is not mentioned and I would like to ask Mr. Knight whether he does not find that the volume of free carbon present is always a considerable factor. He also

mentions a metalloid that we very seldom hear of or take into consideration—our ordinary analysis not taking it into consideration—and that is titanium. I think those that have to deal with analysis in our mixtures are bothered to face conditions which our previous experience with the elements of which we have analysis, silicon, manganese, sulphur and carbon does not account for; and it has often seemed to me that some other elements were present and exerting an influence of which we had no knowledge; and I would like to ask to what extent titanium, or other metalloids, are present in foundry iron, or liable to be present, and what their direct effect is; that is one question, and the other is with reference to the volume of carbon present, whether that does not also affect the shrinkage?

Henry Gossiger: Our first charge is 2,000 pounds. Whether we are wrong or right we **certainly** should not use any part of the first tap out of that first charge on any particular castings. What the reasons are for it I cannot explain; I am not up in chemistry. It may be that it is all fallacy, but our experience has been that the first 1,000 pounds taken out of the cupola are very unreliable, as far as hardness is concerned.

Lincoln Haney: Our first tap out of the cupola—first 400 to 500 pounds—we generally put into castings which will not require much finishing. We don't regard this iron as being of an extra quality.

Mr. West: The complaints on the ground of castings being too hard are numerous. The founder is often called up by telephone by the machine shop and the words will come: "What is the matter with that iron? It don't plane. The machine takes four times as long as it should." After we get out of the grade defined as "soft" we have a very large lee-way. We have a very wide range in difference of textures before we will find any fault is complained of. So, for that reason, the term "uncertainty" as has been applied here would mean, according to my understanding, that it was a hard iron, and I think that researches on that line would bear me out that it is strictly in reference to iron not being sufficiently soft that we mainly have complaint.

Mr. Knight: There are a few points brought forward in this discussion upon which I would like to touch briefly. The first is the melting point of various grades of iron. Some time ago our people went to considerable expense in their laboratory, in order to determine absolutely the melting point of various grades of iron. We selected one brand which was supposed to be about as homogeneous as any brand we could possibly get—the Embreville—and we took one sample of what is known as “soft iron” and another of what is known as white iron to determine the actual melting point; and the soft iron melted at a temperature of about 100 degrees C. below that of the white iron. The temperatures were 1050 degrees C. for the “soft iron” and 1150 degrees C. for hard iron—possibly I may be a few degrees out either way. In regard to the influence of various metalloids upon the shrinkage of iron, I would say in reply to one of the questions asked that certainly free carbon does exert a very great influence upon the shrinkage of iron; but I have taken into consideration the fact that the amount of free carbon contained in an iron is very nearly in proportion to the amount of silicon it contains; consequently, we leave consideration of the free carbon out entirely in enumerating the numerous elements. In regard to titanium, I would say that it is often supposed that a very small percentage of sulphur will materially increase the shrinkage of iron. Titanium compared with sulphur is a very potent element and I have known cases where a pig of ordinary section which contained about .12 per cent of titanium could scarcely be broken by a sledge hammer; while without the titanium the pig could be broken very easily with a hammer of light weight. So much for the influence of titanium. One other point. That is the possibility which has been spoken of that local conditions might have affected the observations I have made. In regard to that, I would say that the observations which I made were taken in our own foundry, and we were melting down there about 300 tons a day and we were doing it in two cupolas; consequently, you can see if there were any possibility of a technical conventionalism to come into play, that it would not be so much in such a case as that. Furthermore, we are using cupolas which

have a capacity of about 20 or 30 tons an hour; and the hearth alone is capable of holding 10 to 12 tons of molten iron before tapping out. So when it is first tapped, I mean the first five or ten tons which are melted—

Mr. Moore: I think that the explanation given of the conditions under which the first tap was made emphasize the fact that there were peculiar local conditions present. Most of us don't melt 300 tons a day, nor on such a large scale; so that the facts that may prevail in an ordinary foundry, as regards the early part of the heat, might not be true where the operations were conducted on such a large scale. I would like to restate my question in regard to titanium. Is that a metalloid which we may come in contact with frequently in pig iron to an extent which would affect the product?

Mr. Knight: I don't suppose you will find one brand or grade of iron among a hundred free from titanium; very nearly always the titanium in pig iron will be between .4 per cent and .12 per cent. The possibility of any cases being affected by the contents of titanium is, as I have stated, quite large, on account of the extremely small percentage which it takes in order to not only increase the closeness of the grain, but also to affect the strength of the material. In regard to the conditions under which we melt, I failed to state that we use absolutely no charcoal; and as to the conditions not being the same as in smaller plants, I don't see how there would be any material difference, because the melting heat would be about the same, only possibly we use coke which would be freer from sulphur than that which is ordinarily used. We use coke which carries about $87\frac{1}{2}$ per cent of fixed carbon and a little less than .6 per cent sulphur.

Mr. West: Mr. Knight's remarks open up a new field. If he had let us know in the start that he was tapping out so much iron, we would have started out on a different basis. He was speaking of one thing and we of another. There is no doubt on such a large tap as Mr. Knight speaks of that he did get softer iron, for the simple reason that it melted quicker than

iron that followed it. We were all discussing the first 200 or 300 pounds which could be got out, so no doubt we both were right on that point. One other point I would like to call attention to is in regard to the use of titanium. Mr. Knight referred to a test of breaking pigs. I should not be guided by that. If we are to be guided by such a test as that, we are to say chemistry is of no value to us, and we would be right in judging iron by its fractures. In order to know the true value of an iron you must remelt it. If Mr. Knight has any experience or tests which he has followed out in titanium, in melted iron, then we would be on the right road to know whether it was really an element of strength or not. I would like to ask his experience in that line.

Mr. Knight: You will understand that in speaking of the test I simply used that as an instance in point, because it covers a noticeable fact in our experience there, noticed in chemical analysis and otherwise. We very frequently take contracts that call for a very high transverse strain, and we use titanium to bring up the strength, and a bar which will carry .12 per cent titanium will always give us very high strength. We buy pig iron which contains titanium. Certain brands contain more than others, according to their analysis.

A vote of thanks was then given Mr. Knight for his very able and interesting paper.

The members and visitors attended a theater party at Pike's Opera House in the evening, on the invitation of the Cincinnati contingent, and had a very enjoyable time.

TUESDAY MORNING SESSION.

The meeting was called to order at 10:30 a. m., President Sercomb in the chair.

On motion of Mr. S. T. Johnson, the association adopted the monogram used on the badges worn at the meeting as the official monogram, and tendered a vote of thanks to Mr. Morris for the idea.

The reading of Mr. Thos. D. West's paper on "Comparative Fusibility of Foundry Metals" followed. Mr. West illustrated by a blackboard sketch the form of cupola used, and designs of castings involving the principle of shrinkage.

"COMPARATIVE FUSIBILITY OF FOUNDRY METALS."

By THOS. D. WEST.

The first paper treating of this subject was read before the Pittsburg Foundrymen's Association, June 28, 1897, and was published in full by *The Iron Trade Review*, July 1, the *August Foundry*, the *Journal of the American Foundrymen's Association*, Vol. 3, Nos. 13-14 (also the *American Manufacturer*, July 2), and *London Engineering*, September 10, and reviewed by the *Engineering Magazine* for September. The first paper illustrated the writer's comparative fusion test cupola and gave among other things, a series of tests which prove that iron made hard by reason of low silicon and high sulphur will come down faster, or require less heat to melt it, than iron made soft by reason of high silicon and low sulphur, when the carbons are alike and all conditions are the same as regards the working of a cupola.

This paper,* aside from presenting several important discoveries, shows that a chilled body of iron will melt faster and require less heat than a gray body, both having been poured from the same ladle or cast of iron, or in other words, being of the same chemical composition, aside from differences in the relative proportions of combined and graphitic carbon due to the rate of cooling. It presents a series of experiments proving that steel proper requires higher heat than cast iron to fuse it, and that remelting of steel in contact with incandescent carbon wholly destroys its original character.

Comparing the fusibility of gray and chilled bodies both of the same composition, was suggested by Dr. Moldenke, the experienced metallurgist of the McConway & Torley malleable iron works, at the Pittsburg foundrymen's meeting last June. To do this, the writer has designed the following simple plan: A heat

of chilling or No. 3 charcoal iron, designated as heat No. 9, Tables 3 and 4 (the iron being furnished by Dr. Moldenke), is caught in hand ladles and then poured into sand and chill molds placed side by side. This gives a wholly gray body of iron in the casting coming from the sand mold and a wholly chilled or white crystallized body of iron from the chill or all-iron mold, both, it is to be remembered, being from the same ladle of iron.

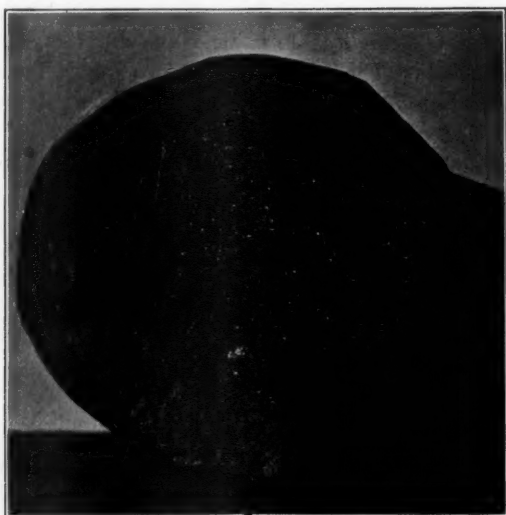


FIG. 1—GREY ROLL.

Combined Carbon, 1.20.

Graphitic Carbon, 2.90.

There are several very important features shown by the results in Tables 3 and 4, aside from proving that a chilled iron will melt faster or require less heat than a gray iron of the same composition. Before proceeding to discuss these features, attention is called to the fact that Dr. Moldenke corroborated the results given in Table 3 by testing the comparative fusibility of chilled and gray bodies of the same iron in an open-hearth furnace, which he did with rolls cast from heats Nos. 9 and 11, Table 3.

TABLE 3—COMPARATIVE FUSION TESTS OF GRAY AND CHILLED IRONS.

Heat Numbers	9	10	11	12	13	14	15	16	17	18	19	20
Kind of metal charged.	Charcoal pig iron.	Sand and chill rolls.	Sand and chill block re-melt.	Sand and chill rolls.	Charcoal pig iron.	Sand and chill rolls.	Charcoal pig iron.	Sand and chill rolls.	High Manganese and phosphorus charcoal pig iron.	Sand and chill rolls.	Foundry pig metal.	Sand and chill rolls.
Weight of iron charged each side of cupola75 lbs.	48 lbs.	44 lbs.	32 lbs.	70 lbs.	53 lbs.	77 lbs.	58 lbs.	68 lbs.	40 lbs.	73 lbs.	55 lbs.
Blast put on1:30	4:06	3:38	2:23	1:10	2:30	1:05	1:47	3:49	3:56	4:22	4:25
Chilled iron running	4:13	2:35½	2:38	1:52½	4:01	4:35
Gray iron running1:39	4:15	4:11	2:39	1:18	2:40¼	1:15	1:54	3:57	4:01¾	4:33	4:35½
Chilled iron all down	4:21	2:44	2:51	2:02	4:08	4:38½
Gray iron all down1:47	4:22½	4:22	2:47	1:26½	2:52¾	1:27	2:03	4:06	4:09¼	4:42	4:39¼
Chilled iron exceeded gray in starting	2 m. 3m. 30s.	2m. 15s. 1m. 30s.	45 s.	30s.
Chilled iron exceeded gray in finishing	1m. 30s. 3m. 1m. 45s. 1m.	1m. 15s.	45s.

Figs. 1 and 2 show fractures of gray and chill rolls taken from heat No. 10, and are from photographs taken by Dr. Moldenke, as also are those shown in Fig. 3. To emphasize the value of Dr. Moldenke's fusing tests, I would refer to my first paper with his discussion appended, showing just how he makes comparative fusing tests in the open-hearth furnace. It will be well also to mention that the gray or sand rolls which have been used in all these comparative tests were all tumbled so as to get the sand off them thoroughly before they were charged.

Before discussing the results shown by Tables 3 and 4, it will be well first to clearly describe the plan observed in carrying through the eight heats shown.

TABLE 4—CHEMICAL ANALYSIS AND SPECIFIC GRAVITY OF GRAY AND CHILLED IRONS.

Heat Nos.	9	10	11	12	Analysis of castings obtained from the 12th heat.						
Kind of metal charged	Char. coal pig iron.	Sand rolls.	Chill rolls.	Sand rolls.	Chill rolls.	Sand rolls.	Chill rolls.	Sand rolls.	Chill rolls.	Sand rolls.	Chill rolls.
Analysis letter. . .	A	B	C	D	E	F	G	H	I	J	K
Total car- bon	3.94	4.10	4.06	4.30	4.30	4.47	4.40	4.68	4.62	4.76	4.70
Graphite carbon . .	3.06	2.90	0.16	2.42	2.68	2.90	0.20	2.67	0.00	3.16	0.03
Combined carbon ..	.88	1.20	3.90	1.98	1.62	1.57	4.20	2.01	4.62	1.60	4.67
Silicon82	.75	.75	.63	.08	.66	.63	.57	.56	.59	.57
Sulphur.02	.03	.03	.04	.035	.04	.04	.045	.046	.048	.044
Manganese.	.78	.66	.66	.53	.54	.31	.33	.18	.19	.25	.22
Phos- phorous..	.232	.248	.240	.274	.285	.237	.254	.254	.250	.271	.266
Specific gravity ..	7.01	7.30	7.61	7.35	7.40	7.70	7.47	7.76	7.46	7.79

For heat No. 9, charcoal was charged in both chambers or shafts of the cupola and run out of one tap hole, the metal being poured into sand and chilled molds from one ladle. For heat No. 10 the sand and chill rolls from heat No. 9 were charged in their respective sides and the two tap holes used. The melted iron as it ran from this heat through open tap holes dropped into sand

molds, one being set under each tap hole, to give a block of iron from each side about six inches diameter by six inches high. This tenth heat had both sides run into sand molds, instead of having sand and chill molds used as shown in heats Nos. 9, 11, 13, and 15, for the purpose of learning which would be the harder iron when remelted, that which had been chilled or that which had not. Heat No. 11 melted down the gray blocks obtained from heat No. 10 and this iron was again run into sand and chill molds or roll castings. Heat No. 12 was a remelt of the sand and chill rolls obtained from heat No. 11 and was the fourth and last heat of a continuous remelt of the original charcoal pig used in heat No. 9. Heats 13 and 15 were casts in which the same grade of

TABLE 5—CHEMICAL ANALYSIS OF CHILLED AND GRAY IRON RE-MELTS.

Description of Iron and heat No.	Analysis of gray blocks obtained from 10th heat.		Analysis of gray blocks obtained from 16th heat.		Analysis of gray blocks obtained from 18th heat.		Analysis of chilled rolls charged on 20th heat.		Analysis of gray blocks obtained from 20th heat.	
Classification of re-melts.	Sand roll re-melt.	Chill roll re-melt.	Sand roll re-melt.	Chill roll re-melt.	Sand roll re-melt.	Chill roll re-melt.	Sand rolls as charged.	Chill rolls as charged.	Sand roll re-melt.	Chill roll re-melt.
Analysis letter...	A2	B2	C2	D2	E2	F2	G2	H2	I2	J2
Total carbon....	4.30	4.30	4.30	4.30	2.94	3.15	3.55	3.60	3.88	3.95
Graphite carbon..	2.42	2.68	2.20	3.20	2.41	2.73	2.63	2.05	2.15	2.40
Combined carbon	1.98	1.62	2.10	1.10	.53	.42	.92	1.55	1.73	1.55
Silicon63	.68	.75	.87	.55	.60	1.55	1.57	1.29	1.39
Sulphur04	.035	.04	.035	.045	.048	.030	.030	.042	.040
Manganese53	.54	.48	.62	1.23	1.32	.133	.135	.126	.130
Phosphorus274	.285	.283	.241	1.07	1.07	.343	.330	.364	.350

pig was used as in heat No. 9 as a check to learn if similar results would be obtained by extended experiments, and heats 14 and 16 are used as a check on heat No. 10, in the same manner.

The analyses given under A, B and C, Table 4, for heats Nos. 9 and 10 will also serve for heats 13 to 16. When running the 16th heat the sand and chill rolls metal was run into sand molds the same as described for heat No. 10. Heat No. 17 is a high manganese and phosphorus pig, which was run into sand and chill molds to make rolls that were used for heat No. 18, from which

the gray and chilled metals as they came down were both run into sand molds to further test the question whether a remelt of chilled iron would not be as soft as the gray iron of the same grade. Heat No. 19 is a No. 2 foundry all coke iron, which was also run into sand and chill molds. Heat No. 20 is made from the sand and chill rolls obtained from the 19th heat, both of which metals were run into sand molds as heats Nos. 10, 16 and 18. Analyses of the gray and chill roll remelts are all shown in Table 5.

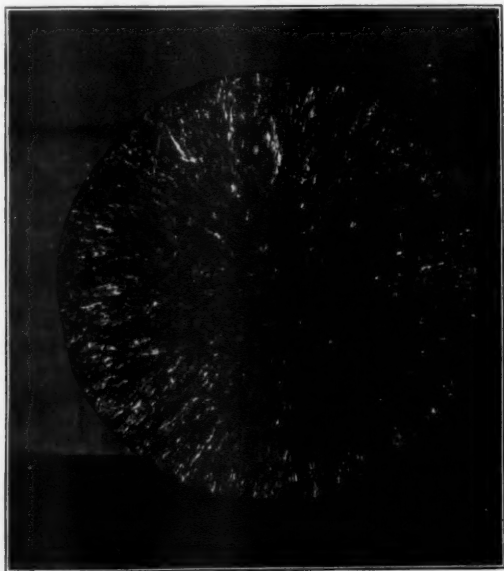


FIG. 2—CHILLED ROLLS.

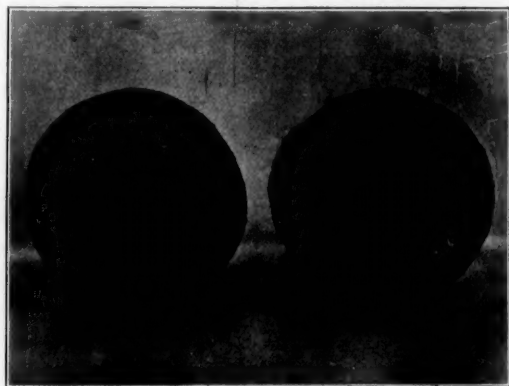
Combined Carbon, 3.90.

Graphitic Carbon, 0.26.

Economy and Advantages of Chilled or Sandless Pig Metal.

It has been the belief heretofore that an iron once chilled would upon being remelted produce a much harder casting than if the same iron had never been chilled. This belief is so strongly maintained by foundrymen that in selecting scrap iron for mixtures

with pig metal to make light or heavy machinery castings, etc., some will reject the scrap that has been chilled, if it can be done, lest it might cause hard spots in a casting or make the whole too hard. Of course it is to be understood that if a casting shows a chill, it is evidence that the gray body of the casting, if used for scrap, is not accepted as a soft iron, as if no part of the casting exhibited a chill; for, as a rule, founders know such fractures are not to be graded as soft iron. Nevertheless, they do not know, as far as the writer is aware, that instead of a chilled iron body giving a harder remelt, it actually gives the reverse results, so as to make a casting slightly softer by having the chilled scrap used than if it



GRAY ROLL. FIG. 3. CHILLED ROLL.

had been rejected and only the gray body utilized. While this knowledge would always have been of much value to the founder, there has been no time that it could be turned to more profitable account than at the present.

There are a few progressive founders that are not aware of methods now used by two Pittsburg blast furnaces whereby the casting of pig metal is done in iron chills instead of sand molds. These methods were described in the *Iron Age*, *The Iron Trade Review* and *American Manufacturer* recently. Three or four years back such a product

of chilled pig metal would be rejected by almost all the foundry trade simply because we then judged pig metal by fracture; and if anyone then even suggested charging pig metal that was chilled, or partially white, thinking thereby to obtain a gray casting, he would be considered insane. But thanks to chemistry and the good work of our foundrymen's associations and co-workers, we now know fractures are deceptive and that the true character of pig metal is only shown by analysis. It may be asked what evidence there is aside from the drilling tests to prove that the chill roll remelt was softer than that of the gray. This is answered by referring to the columns B2, D2, F2 and J2, Table 5, and noting the greater silicon and graphitic carbon existing in the chill remelt than is found in the gray. The writer's attention was first drawn to the fact that the chill remelt was softer than that of the gray, in drilling to obtain material to make the analyses. The drill worked so much easier in the chill remelt than in the gray as to be matter of much surprise. To make sure the touch was not deceptive, others were asked to try the drilling, and they all concluded the chill remelt drilled softer than the gray in the first two casts. The drilling test seemed to be very important, as one element in testing the hardness of the iron. We therefore arranged that future tests could have something more reliable from which to record results than the mere sense of touch. The drill press used is of the type worked by a lever to feed the drill. On the lever we affixed a 15-pound weight at a given point, which was notched so that the weight would always be placed in the same position. After a well sharpened twist drill was attached and all ready, the drill was started and allowed to run exactly half a minute. By drilling several holes in this manner, alternately in each of the respective blocks, we could then, by measuring their depth, intelligently tell which of the two was the softer. This process was followed in testing the blocks coming from heats Nos. 18 and 20, which were made partly as checks on the first two heats, Nos. 10 and 16. It is to be said that the drillings of the whole four heats, Nos. 10, 16, 18 and 20, showed the chill remelt to be softer than those of the gray iron. It will be noticed also that these four remelts are distinct in testing different grades of

iron, so as to cover a wide range of metals, from those that would take but a slight chill on the surface of pig metal or a casting up to those that would chill its whole body as displayed in Fig. 2.

The economy and advantages to be obtained by using chilled pig metal in foundries and in Bessemer steel works may be stated as follows: First, being a harder iron by reason of its chill, or having its carbon largely in the combined form, as well as having the pigs free of sand, less fuel will be needed to melt it, and it can be made to come down faster in the cupola, thus saving time in melting. Second, the pigs being sandless there will be needed less fluxing and slagging of a cupola in large heats and this will

TABLE 6—COMPARATIVE FUSION TEST OF CAST IRON WITH OPEN HEARTH STEEL.

Heat Nos.	21	22	23	24	25	26	27	28
Kind, weight and form of metal charged each side.	35 lbs. of iron and steel in block form.	29 lbs. of iron and steel in block form.	23 lbs. of iron and steel in block form.	61 lbs. of iron and steel in scrap form.	44 lbs. of iron and steel in block form.	52 lbs. of iron and steel in scrap form.	40 lbs. of iron and steel in block form.	58 lbs. of iron and steel in block form.
Blast put on..	8:56	2:50	3:00	11:30	3:23	10:37	11:29	3:26
Steel running.	9:09½	2:59½	3:05	11:41¼	3:32	10:45¾	11:38¼	3:32
Iron running..	9:06½	2:58	3:04	11:38	3:30½	10:45	11:36¾	3:32¼
Steel all down	9:21	3:08	3:11¼	11:51	3:41½	10:58½	11:49¾
Iron all down.	9:17½	3:05½	3:10	11:48	3:40	10:58	11:49
Iron exceeded steel in starting	3m.	1m. 30s.	1m.	3m. 15s.	1m. 30s.	45s.	1m. 30s.	15s.
Iron exceeded steel in finishing	3m. 30s.	2m. 30s.	1m. 15s.	3m.	1m. 15s.	30s.	45s.

also give a cleaner iron out of small as well as large heats from the tap hole. Third, its being chilled will cause it to be broken more easily.

These are the benefits to the founder and steel-maker. The furnaceman is also benefited. There is a considerable saving in making iron, and casting in chills dispenses with the most arduous labor that man can do. Anyone watching iron breakers walking over a semi-molten bed of pig metal, breaking it from the

sows and then loading it for carriage out of the casting house, to make room for another cast before the metal is cold, will appreciate this. There is nothing that troubles furnace managers more than to find and hold men that will stick to such laborious work, for if a man lasts ten years at it, he is generally of very little value afterwards for other laboring work. Taking all together it is to the interest of all to use chilled cast pig metal, instead of the sand bed pig, as at present.

Chemical Changes Due to Re-Melting.

Attention is called to the fact that this paper contains 162 analyses, ten specific gravity tests and several fracture views which have all been made and kindly furnished by Dr. R. G. Moldenke.

TABLE 7—CHEMICAL ANALYSIS OF GRAY CAST IRON AND OPEN HEARTH STEEL RE-MELTS.

Heat Nos.	21		22		23		Analysis of metal obtained from the 23d heat.		Analysis of metals charged in heats Nos. 24 to 26.	
Kind of metal charged.	Gray cast iron.	Open-hearth steel.	Gray cast iron.	Open-hearth steel.	Gray cast iron.	Open-hearth steel.	Gray cast iron.	Open-hearth steel.	Gray cast iron.	Open-hearth steel.
Analysis letter.	L	M	N	O	P	Q	R	S	T	U
Total carbon...	4.02	.60	1.48	2.74	4.60	3.05	4.20	.70
Graphite carbon	2.90	3.30	.15	3.03	trace
Combined carbon	1.12	.60	1.48	2.74	1.30	2.90	1.17	.70
Silicon	1.72	.312614	1.15	.35	1.24	.38
Sulphur03	.0261014	.10	.18	.05	.12
Manganese35	.342315	.23	.06	.40	.59
Phosphorus....	.073	.106167190	.103	.198	.092	.116

In taking up a study of Table No. 4 we are first struck by the increase of total carbon. We find that starting with the original pig containing 3.94 carbon, four remelts increased it to 4.76, an increase of nearly one per cent. It is to be noted that in all cases the sand or gray rolls show more carbon than the chilled roll.

The next feature of importance is the relative proportions of graphitic and combined carbon in the sand and chill roll remelts. We find that by changing the rate of cooling the same iron can produce one casting holding the carbon in a graphitic form as high as 3.16, while another casting from the same ladle has practically no graphite in its composition, but holds the carbon mostly in a combined form, as is seen in columns J and K, Table 4. The writer would call special attention to the difference existing between the gray iron and chilled roll carbon determinations. This would not bear out the claim that no difference exists in chemical composition of the gray and chilled portions of the same casting. We next come to the silicon, sulphur, manganese and phosphorus. We find these are all in line with the varied experience of those who have kept close watch of results obtained by remelts, to the effect that silicon and manganese decrease while sulphur and phosphorus increase. It may cause some surprise that more silicon was not lost or sulphur added than shown by the four continuous remelts in heats 9, 10, 11 and 12, Table 3. The only way the writer can account for this is that the metal was not held very long in the cupola during the melting process compared to the time generally occupied in ordinary shop practice, in which a charge of iron is something like an hour in passing down from the charging hole to be tapped out as fluid metal. In the heats shown, Table 3, the iron is not exposed to the effects of the blast and gases longer than 24 minutes and in most cases only from 16 to 18 minutes. When we consider that the longer iron stays in contact with incandescent fuel or is exposed to gases, the more sulphur will be absorbed up to the limit of the iron's affinity for it, we can readily reason out why the increase of sulphur is not more than that shown. The reverse is true of silicon; the longer iron is exposed to the effects of the heat and blast, the more silicon is lost.

Semi-Steel Castings.

Regular steel requires longer time to fuse, than is necessary for cast iron, as will be seen by Table No. 6, in which heats Nos. 21, 22 and 23 are continuous remelts of the same metals. The steel was a "riserhead" piece of scrap that was molded to make a

single piece of cast iron of the same form so that conditions as to form and weight could be the same for both metals in making the comparative fusing test shown.

Heats 24, 25, 26 and 27 are two remelts of different quantities of cast iron and steel metals, having similar composition, as will be noted by referring to columns T and U, Table 7.

Heats 24 and 26 had the metals in scrap form as nearly alike in size and bulk as they could be roughly made and when melting they ran into molds to give blocks two and a half inch by four inch by six inch, so as to insure a uniform size of stock for making the comparative heats 25 and 27. Heat 28 was a remelt of the blocks obtained from heats 25 and 27. This heat, it will be noticed, had the iron and steel come down closely together. The reason the closing time is not shown is on account of stopping up the tap holes after the iron had started to run, with a view to catching metal in a hand ladle to pour shrinkage and contraction tests, which left the matter too indefinite to record the time of actually finishing first, although, as near as we could see or judge, they ended closely together. It will be noticed by a study of Table 6 that the more we remelt scrap steel the less difference exists in the iron starting and closing ahead of the steel. This is no doubt due to the fact that remelting steel raises its carbon and at the same time we find that steel remelt will be very spongy or filled with gas or blow-holes, which increase more in size and number with each successive heat, thus causing the steel product to be very porous and thereby permitting the heat to better penetrate its body and bring it quicker to a fluid state.

Table 7 plainly shows the uselessness of trying to remelt steel and obtain from it the original metal, as can be done with cast iron. Nothing has led founders on more wild-goose chases than giving ear to some of the high-sounding claims made for remelts of steel or its mixture with cast iron. It is true that steel scrap mixed with cast iron can strengthen the latter to a limited degree, but the extreme claims some make for it and for its mixture are erroneous and unfounded. We have no metal that will deteriorate from its original state by reason of remelting, so much as steel scrap. The action taking place in remelting steel in a cupola

increases the carbon in the metal, as shown in Table 7. We find that the first remelt raised the carbon from .60 to 1.48; the second sent it up to 2.74 and the third to 3.05—an increase in either of these three remelts sufficient to show we are very far from retaining anything like the original steel in any remelts in a cupola, which compels the steel to be in contact with the fuel from which it absorbs the carbon with avidity.

When steel is melted in a reverberatory or air furnace in mixture with cast iron, we have more favorable conditions, because of its being possible to keep the carbon down and add other metals, as spiegel and ferro-manganese, which alloy with the fluid metal without having their original properties destroyed to any large degree. Tensile strengths ranging from 45,000 to 50,000 lbs. per square inch have been obtained by air furnace meltings with mixtures of iron, steel, etc., but to obtain castings equal to those of steel proper, we must have them cast by regular steel foundries. Wherever we desire to improve the strength of cast iron by mixture with steel, the lower carbon steels will be found to give the best results, and air furnace meltings excel those of a cupola, especially if charcoal irons are used. In mixtures with the latter, from 15 to 30 per cent of soft steel scrap may often be advantageously used.

Shrinkage and Contraction and Specific Gravity of Cast Iron.

Usually if a mold is poured short where the weight of metal has been figured, the first thought is that some mistake has been made in figuring the cubic contents or volume of the mold or ladle, and after all figures are checked over again and due straining and extra metal allowed for, and are still found correct, the question where the iron went is farther than ever from solution. Such a state of affairs is often met, and frequently causes heavy losses. For some time back the writer has felt that specific gravity and shrinkage of molten metal often have something to do with the question, "Where did that iron go to?" The writer's experiments in testing the comparative fusibility of metals, in handling different grades at the same time, have led him to the belief that there occurs a shrinkage of molten metal under certain

conditions that we have not heretofore known or recognized. Realizing the advisability of measuring in some way this shrinkage, research was made without success, to find if any methods existed whereby the specific gravity of molten metal could be obtained, so as to make a comparison with the same when the



FIG. 4.

metal had solidified. The writer then devised the following original method, which can not be called a specific gravity test, but more a measure for volume.

At M, Fig. 5, is seen an iron pattern from which sand or chill molds may be made. At A, Fig. 4, is an iron box three inches

square by 11 inches long, in which the pattern has been molded to make a dry sand mold. This mold, as shown, is filled with hot molten metal. The cut shows a molder in the act of pouring the contents of the mold into a chill or all-iron mold. This is split in halves, as will be noticed, and a ring clamp, as at B, is used to hold it firmly together, E being a bottom block for the

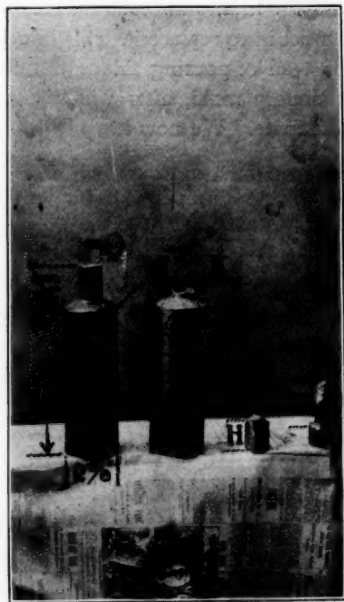


FIG. 5—SHRINKAGE PATTERN AND TEST CASTING.

chill proper to rest on, and D a funnel cap placed loosely on the top of a chill to insure the stream of metal being guided directly into the chill mold without any being spilled. Before pouring these molds they are tested to learn if their cubic contents for holding metal are exactly alike, by means of filling one with fine hour-glass sand and then pouring the same into the other. This is done only as a precaution to make sure no extra thickness of

blackening or distortion of the dry sand mold had occurred in any manner while making it. There are three of these dry sand molds made for each cast or test of any one grade of metal, two being called portable and one stationary. The plan of using these molds is as follows: A portable mold is secured in the ladle shank and the cupola tapped to fill it direct, and it is then quickly poured into the chill mold as above described and seen in Fig. 4. This done, the first sand mold is removed from its ladle shank and another set in to replace it. This in turn is again filled with metal and instead of pouring this into a chill, it is poured into the sand stationary mold, after which it is then removed and placed with its mate. We now have two molds, one a chill

TABLE 8.—SHRINKAGE AND CONTRACTION OF GRAY AND CHILLED IRONS.

Heat Nos.	29	30	31	32	33	34
				15% steel		
Character of metal tested.	Ferro-silicon.	Foundry iron.	Bessemer iron.	with gray iron.	Charcoal iron.	Charcoal iron.
Silicon	12.25	1.75	1.72	1.61	.75	.70
Sulphur021	.04	.054	.055	.03	.035
Shrinkage of 3 oz.	2 oz.	2 oz.	2 oz.	2 oz.	6 oz.	6 oz.
chilled iron..	240 gr.	240 gr.	180 gr.	290 gr.		280 gr.
Shrinkage of gray iron....	3 oz.	1 oz.	1 oz.		1 oz.	2 oz.
Contraction of chilled iron....		210 gr.	140 gr.		460 gr.	120 gr.
Contraction of gray iron270"	.262"	.271"	.322"	.446"	.460"
Contraction of gray iron24 "	.205"	.211"	.227"	.229"	.235"

and the other a sand mold, that will have a sunken space at the neck K, Fig. 5. To learn the amount of shrinkage that has taken place, the shrunken spaces at the necks of the chills and the dry sand molds are now when filled with molten metal taken from the molds cold, and separated from the main casting, views of which pieces are seen at E and H, Fig. 5. The straight portion or H is that created by the shrinkage, which takes place as the metal is being poured before the mold is filled; and the portion at E, which is irregular in outline, is that created by the shrinkage of the molten metal in cooling to a solid after the molds have been poured or filled. A little study of the sections

E and H will show that their total weight (by fine apothecary scales), minus any thin wafer sheets of iron that might be found sticking to the walls of the dry sand mold that had not run out as metal to test the shrinkage, would be the shrinkage of that iron under the conditions in which it had been poured. Table 8 gives the shrinkage of a number of metals which the writer run specially to test the subject of shrinkage and contraction and which were mostly distinct heats from those shown in Tables 3 and 5, making all told 24 heats described in this paper.

By referring to the following table, it will be seen we have in castings measuring about two and a quarter inches diameter by seven inches long (the actual form and size being seen at M, Fig. 5), weighing nearly eight pounds, a shrinkage in the chilled iron of about six ounces and in the gray about two ounces. This means a shrinkage of about four and a half lbs. per hundred for all-chilled iron or rolls, Fig. 2, and nearly two lbs. per hundred for the gray iron or rolls, Fig. 1. In larger figures, for a 20-ton casting, the table would imply a shrinkage of about 1,800 pounds for chilled iron, were it possible for all of its body to be as thoroughly chilled as are the rolls seen in Fig. 2, and 800 pounds for the gray iron if the total body of the casting does not get up in graphite any higher than the rolls hold it, as seen in Fig. 1.

It is to be remembered that the tests of iron shown in Table 8 do not include an iron as soft as is necessary for stove plate or very light castings, and because such grades of iron are softer than any shown in Table 8, it is to be said that such softer grades would not have the shrinkage that is shown above. Because we generally run this iron into very thin castings we have reason to expect that the shrinkage of the molten metal during the action of pouring stove plate, etc., will be for such work greater in ratio than the irons noted in Table 8, when poured into heavy castings. The part at H, on the left of Fig. 5, exhibits the shrinkage that takes place as the mold is filling up, while E on the right is the shrinkage that takes place after the mold is filled and the iron solidified. The latter is also shown by the shrinkage hole at the right of Fig. 3, which is chilled iron made from the same ladle that the roll on the left of Fig. 3 was poured from, and which

does not exhibit the internal shrinkage shown in the chilled roll, because of being allowed a longer time to cool, being cast in a green sand mold. The shrinkage exhibited by the irregular outlined body at E, right of Fig. 5, which leaves shrink holes in castings similar to that shown in the chilled roll at Fig. 3, is always to be found in that part last to solidify, as the part first to solidify draws from it metal to supply its shrinkage. If this is kept in mind, it will assist the founder to obtain solid castings and overcome many difficulties and losses often incurred by the shrinkage of iron. As an example, the writer would cite a case of the casting of a locomotive pump weighing about 200 pounds that nearly went the rounds of all the founders in a large city some years back, all trying their hands at getting a solid casting. The trouble lay in that there was one part of the casting which had a valve seat bearing in it much heavier than other parts of the casting, which, if not found solid, condemned the casting. It is safe to say that half a hundred of these castings went to the scrap pile before a good one was obtained. As the different shops tried them they would be made in dry sand, skim gated, cast on end and made in most every conceivable form known to founding until finally the writer was asked to try his hand at it. After studying the design of the pump, the writer soon saw that making it in dry sand or anything else combined with casting on end and skim gating it to death would not help matters. If it had been simply molded in green sand after common fashion, and a feeding head or riser had been set right directly on the valve or heavy portion of the casting, large enough to insure the metal in the riser being held in a liquid state longer than that in the valve portion of the casting, no more trouble would have been experienced. By using this plan the writer had no difficulty in making the pump casting solid. An adoption of the principle just outlined can save many castings that to-day are giving many founders trouble and losses. The tests exhibited by the cuts at Figs. 3 and 5 and Table 8 demonstrate that metal will shrink to cause trouble by leaving holes in the interior of castings as well as deceiving a founder as to the amount of metal he should have to pour a casting, and also

that we are to expect the greatest shrinkage in the harder grades of iron. The shrinkage exhibited by the 12 per cent or ferro-silicon iron at heat No. 29, Table 8, surprised the writer in being so high.

Contraction.

There are two distinct actions that control the cooling down of molten metal to a cold state, which are so different and

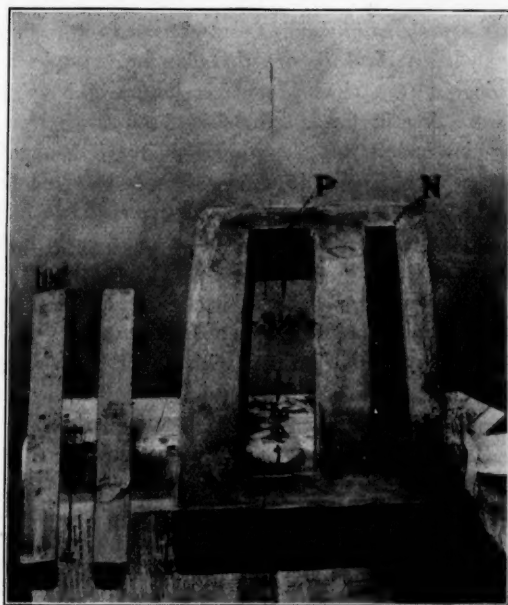


FIG. 6—CONTRACTION CHILL AND SAND TEST MOLDS, WITH PATTERNS.

definite in their effect on the strength of castings, that we should by all means have different terms to designate them. For this reason the writer has accepted the terms "shrinkage" and "contraction." Tersely stated, "shrinkage" implies the action taking place when the metal is being poured and after the mold is filled, in its cooling down to a state of first solidification, causing holes

in castings. The term "contraction" includes that action occurring after the liquid metal has solidified, to cause castings to be smaller in their dimensions than those of the pattern from which they were molded. Each of these actions is distinct in its power to influence the strength of castings, which cannot be better displayed than in the bursting of fly wheels. The shrinkage will often have left cavities where the arms join the rims and the contraction makes trouble because of the internal strains caused by the difference in rate of cooling between the heavier rims and the lighter arms. Light bodies contract much more than heavy ones, to such a radical degree, that, for an example, a section one-half inch by two inches, 14 feet long will have as much again contraction in the length of its body as a section four inches by nine inches, same length. This is well displayed in the few contraction tests seen in Table 8. We find here that tests Nos. 1 and 6 give us the mean of .127 greater contraction for the fast cooled bars than for the slow cooled ones, each of the same cross section and length, patterns for which are seen at R, Fig. 6.

The greatest difference in Table 8 is .225 and the smallest .030. It is to be remembered that the respective tests seen in Table 8 were cast in their order with the same gate and hand ladle of iron. The cause of such a difference in the contraction of two bars is, as will be seen by Fig. 6 at N, that one is cast in a chill mold and the other in sand, P being the space for molding the sand bar. A study of the difference in contraction which the rate of cooling can cause by the device seen at Fig. 6 is instructive in more ways than one. Take the case of the charcoal iron heats, Nos. 33 and 34, which will illustrate the great difficulties the makers of chill rolls, etc., are confronted with. Here we find that the chilled part of the casting will have as much again contraction as the body of the casting that is not chilled. It is no wonder that chill roll makers experience much trouble with the checking and cracking of the surfaces of chill rolls, due to the excessive contraction of the chilled part, which must leave or pull away from the chill mold, supposed to support its enclosed body of liquid metal, long before it has solidified and which by reason of its head pressure, incased within the body

of the shell which has contracted from its chill or outer support, must be heavily strained to retain its enclosed body of still fluid metal. Another matter which the few chill and sand contraction tests seen in Table 8 show is the urgent necessity of our maintaining as perfect a uniformity as possible in the "temper" and nature of sands for molding test bars which are expected to truly record the contraction or other physical qualities of cast iron. We can see by the chill and sand contraction tests herein recorded how a very slight difference in the dampness or nature of molding sand can effect the contraction of test bars. The smaller they are the worse, of course, are they distorted, preventing a true comparative record of the actual conditions which prevail. The irregularity that must exist in the temper and nature of molding sand has suggested the idea that it may be best to use a chill instead of a sand mold for testing the contraction of iron, as, at the best, the utility of test bars is only for comparisons.

Attention is again called to the specific gravity tests seen in Table 4, which, in four successive remelts, raised the density of the iron from 7.01 in the gray to 7.46, an increase of .45 in density, and in the chilled iron to 7.79, an increase of .78 from the original pig, showing that successive remelts greatly increase the density of irons. Another point to be noticed is that the chilled iron differs about .30 in density from the gray iron in the respective heats shown. In testing the density of these irons by immersion in liquid metal, they exhibited conditions which show that with the same hand ladle of metal we could make two castings, equal in volume, one of which would sink like a stone, and stay down, and the other float like a piece of ice on water, by means of chilling one piece and casting the other in sand. Such a test proves that the specific gravity of solid iron and that of the same iron in a liquid form can differ to a radical degree.

Why Iron Chills.

At a meeting of the American Chemical Society, Troy, N. Y., last December, the president, Dr. C. D. Dudley, in his annual address asked, "Who will first completely investigate the relation

between the chemistry and the chilling properties of cast iron?" The writer believes this paper embodies experiments and results which point to a solution of the problem and which are as follows:

Chilling of cast iron is caused by the rapid solidification of molten metal, preventing the chemically combined or hardening carbon which exists in it as such, from changing to graphite, and thus giving a granular structure to solidified iron. This it would do, if the iron solidified more slowly. Some irons will not chill, because they are so high in silicon and low in sulphur or other hardening elements, that the carbon in the molten metal is forced out of its chemically combined form into graphite or free carbon, according to the rapidity with which the molten metal loses its initial heat or fluidity.

It will be noticed that the above definition refers to the chemically combined or hardening carbon, which exists "as such" in the molten metal as taken from the blast furnace or cupola. This implies that the carbon may exist in some other form than that of chemical combination with the molten iron, which is, of course, contrary to the general acceptance, as authorities hold that the carbon in molten metal is all permanently chemically combined with the iron until it has all solidified. In the light of the writer's recent researches, he is led to differ from these authorities and claim that the carbon is not always permanently held in a chemically combined form with the molten metal. There are two phenomena shown in actual furnace work and founding, that the writer believes sustain the position he takes. One is based on the fact that the hotter or more fluid we pour the metal, the deeper it will chill, a phenomenon first brought out and proved by the writer in other papers. The other fact is to be observed in making soft grades of iron at the blast furnace, where free carbon, or "kish" will often be seen to rise continually to the surface of the molten metal as it cools off, sometimes to such a degree as to fairly cover everything around the pig beds, or ladles of iron, with its flakes, which often act and look like fish scales that have been scattered by the wind. If we accept the hypothesis herein presented, that all carbon does

not invariably exist in a chemically combined form in molten metal, it is then a very easy task to explain why a hot iron should chill deeper than a dull iron, and why graphite often should be seen to rise continually to the surface of liquid metal in soft grades. It is also well to state that the cooler the molten metal



FIG. 7—WEST'S LIQUID BATH COMPARATIVE FUSION TEST.

becomes the more it evolves the carbon to graphite. This is once in a while noticeable in the foundry in the remelting of soft iron, but to see the plainest demonstration, it is necessary to visit the blast furnace when making soft or No. 1 iron, which is, of course, high in silicon and as a rule low in sulphur. If any

one cannot accept the writer's reasoning on this subject, he must, to differ from him, first show where the graphite or free carbon comes from in the making of soft iron and why there is a greater escape of the free carbon the duller the iron becomes, and again, why it is that a hot iron will chill deeper than one poured dull.

Comparative Fusion Testing by Immersion.

Before closing this paper, there is one experiment to be described that the writer has designed, and would like to have founders try. At Fig. 7 is seen a three-quarter inch rod in the hands of a molder, being held over a ladle that has fixed to its end a roll casting made in the mold seen at Fig. 8, the upper half S being all green sand held in a wooden box and the lower a chill



FIG. 8—WEST'S COMPARATIVE FUSION TEST MOLD.

or iron mold, made in half and held together by a ring at T, the whole resting on a bottom block U and the metal being poured in at Q. Now it will be seen readily that a casting made in such a mold would have one-half wholly chilled or body hardened and the other of a softer or more complete gray mixture, which, if held in a bath of molten iron or steel, would be a very pronounced test to assist in showing whether hard or soft grades, etc., of iron when charged into a cupola or air furnace as such would melt the faster. The cut seen in Fig. 7 shows a roll having chilled and gray iron ends, that was made in the mold seen at Fig. 8, which was immersed in a crane ladle bath of liquid iron

to test which end would go the first. The cut at Fig. 7 shows the exact appearance of the specimen as it was taken out of the crane ladle bath of molten metal, just as the chill end V was about to disappear entirely, and which we have found in all cases to melt away five to ten minutes faster than the gray end X. As the question of encouraging the manufacture of chilled or sandless pig by the blast furnaceman, which this paper is advocating, is an important one, the writer would advise all to try this experiment and in doing it many will find themselves surprised at the rapidity with which the chill or body hardened end melts compared to the gray or soft end of the test specimen. These experiments would further demonstrate the economy in fuel and time in melting to be secured by the use of chilled or sandless pig in the foundry. The design of this method while simple and inexpensive, is very satisfactory and effective. There are many ways in which its use could be applied by metallurgists and founders, to test and demonstrate the comparative fusibility of metals immersed in a bath of liquid iron or steel. In using this device, some judgment will have to be used as to the size of the test roll and ladle for its immersion. For a roll of two to three inches diameter a ten hundred pounds or over ladle will be necessary, but rolls about one inch diameter could often be melted down in a bull ladle holding 200 to 300 pounds of iron before the metal would get too dull. These rolls are well made about 12 inches long and are secured by the end of the rod seen curved around it tightly in the center. All sand and scale should be well filed or ground off from the sand end of the roll so as to have it free from foreign matter, the same as in the chilled or hardened end, to make conditions alike in each end as far as possible.

The following interpolations were made by Mr. West while reading his paper:

In connection with matter on page 1, we have found by experiments that the carbon has quite an effect in increasing the fusibility of a metal. You may take two irons apparently soft and with the carbon low in one and high in the other, the one

that has the highest carbon will fuse the first. There has been some confusion on the fusing point question caused by different experimenters not recognizing or taking note of the carbon contents.

In connection with page 6, we find in our experiments that a hard iron will melt or cut down faster than a soft iron. It can be made hard either by a combination of metalloids or by chilling. In either event we have found that it will come down faster. That implies we could use less fuel with hard or chilled irons. In Monday's discussion Mr. Bell spoke of it taking more fuel to melt white iron, which is hard iron. That it should require more fuel to melt this white iron is due to the fact that they want to get it very fluid in order to put life in the metal to hold it while pouring. All foundrymen know that while iron loses life very quickly, and you have to get it out of your ladle very rapidly, and the more fluid it is the better it retains its life. I want to carry that point along so that it won't be confused with the idea that we want to present in the paper in regard to easier fusibility by reason of iron made hard by chilling, as chilled iron and white iron are two different things. Appliances have been arranged at blast furnaces so that the iron is caught in ladles, which are then lifted by cranes up to a level where there is a revolving table that has chill molds on it made of cast iron. The iron from the ladle is poured into these while they travel around and carry it to any point desired. These appliances have been used at blast furnaces instead of letting the iron run out into sand pig-beds. At some places as soon as it is poured a stream of water is put right on the iron and by the time it gets to its journey's end it is dumped into the car, which, of course, saves the necessity of making pig-beds, and gives us the chilled or sandless pig metal mentioned in my paper.

In referring to Dr. R. G. Moldenke, on page 8, Mr. West said: I may state that Dr. Moldenke has been of very great assistance to me in getting up this paper by helping me out with analyses. You can see I give a large number of them. If it had not been for his services in that respect, I doubt if I would have been able to come here with the paper.

In regard to page 9, Mr. West said that Mr. Knight, in his paper, presented analysis to show that he obtains higher silicon at the first part of his heat, with a uniform mixture throughout, than he did later. This is due to the iron melting fast. It is not held so long in the cupola. The longer we hold the iron, the more silicon we lose.

The secretary read the following discussion of the paper from Dr. Moldenke:

I can only add to Mr. West's able paper that open hearth and straight draught furnace men have long been looking for a chilled sandless metal which would enable them to reduce the amount of slag, melt quicker and with less burning out of the silicon and manganese. The advantages of melting faster in the cupola are well pointed out, the lessening of slag, smaller increase of sulphur and less burning out of silicon. In the case of melting of steels or low carbon pig irons, the gradual increase of carbon up to a saturation point has since been verified in the recent publications of the iron and steel institute. Furthermore, the spongy nature of the material Mr. West mentions as obtained in the first steel remelt, is probably due to the absorption of gases allowed by the excessive reduction of the silicon and the manganese contents. The resulting material containing 1.1-2 per cent carbon really makes it a high carbon tool steel, though of poor quality, and this kind of steel is well known to be extremely sensitive to gases while in a melted state. The addition of steel to cast iron mixtures enhances the strength by reducing the carbon contents, thus in a manner closing up the grain of the material. In one respect I would differ from Mr. West, however, and that is with regard to the carbon contents of melted iron. The facts as observed are correct enough, but melted iron has the property of picking up more carbon if allowed than can exist in it when it has solidified slowly and under ordinary conditions. Consequently, graphite is bound to separate out and, rising to the top, form "kish." Graphite in melted iron is, therefore, present only for the time being in a mechanical way, and only when the iron is above the carbon sat-

uration point of slowly cooled iron. To demonstrate this more fully, I would suggest to Mr. West to try the following experiments: To fill his experimental cupola to the top with coke, bring it to an intense heat and put in some No. 3 charcoal iron. This when caught below is to be returned above several times, until a very hot, well-saturated iron is obtained. Now the metal is to be cast into very thin chilled bars, some while the iron is at its full heat, others as it becomes duller. The idea being to get the iron in such a graduation of temperatures and holding its composition intact by rapid chilling, that the formation of graphite while cooling may be studied by means of the microscope. I would gladly undertake this end of the work and present the results in the form of photographs of the actual structure.

John D. Sadlier: I don't know, Mr. President, whether there is any real value in the information with respect to Mr. West's paper that I can suggest to you. There are some things that I certainly do not quite agree with Mr. West in connection with it; but I would not now be prepared to give an intelligent reason for my position; yet his paper is a very able one and brings out many wonderful and deep ideas. There is a particular point I will mention. I have been accustomed for a number of years to make very heavy castings, yet I have never taken the matter into consideration of molten metal being so much larger than the space it will fill afterwards in the mold when cooling. I have endeavored to form some conclusions respecting that matter by calculating by ladle-fuls the probable weight of the total iron poured out into a casting; but my calculations would never hold out. It is something that is new to me and something that I appreciate very much—something I have never thought of before.

Mr. Knight: One or two things I hardly understand in regard to the experiments which Mr. West has performed. He makes several statements, which, although probably very clear, are at the same time in the nature of a surprise to the metallurgical world at large—not possibly because they are so much on the order of something not to be thought of as existing, but rather

because no part of the work had been gone over before. He makes one assertion which was brought out yesterday, I believe by Mr. Bell in his discussion of the paper which I presented, that in order to get a good casting from white iron, it was necessary to heat the iron to a much greater degree of fluidity, because the iron lost its life a great deal sooner than gray iron. I would like to ask Mr. West, also Mr. Bell, if they don't think the very reason why gray iron keeps its life so much longer in the ladle is because the fusing point is so much below that of white iron. You can pour it a great deal colder and produce a harder casting. In fact, a great many experiments entered into by various companies some time since have proved as conclusively as it is possible to do it the melting points of various brands or grades of metalloids. In one experiment we performed, white iron melted at a temperature of 110 degrees above that of gray iron, and the analysis of the two irons very nearly correspond, the only difference certainly being in the silicon and carbon, and the variation in sulphur and manganese being quite small. There is a point Mr. West makes which seems to me a little bit obscure. He states that the longer the iron is held in the cupola the more silicon will be burned out. That goes back again to the reaction which takes place in the cupola from the very nature of the operation, the blast coming into the tuyeres and going up through the fuel and making it incandescent, the iron must necessarily melt a number of inches above the tuyeres, usually about 18 to 20, sometimes only 14; but the fact remains, nevertheless, that the iron melted by that high temperature, must go through the incandescent fuel. There is where the carbon is being absorbed. The carbon is being burned and turned to carbon dioxide. The amount of oxygen you have in the cupola is undoubtedly in excess of the amount required by carbon which is being burned. At the same time you are having carbonic oxide gas coming out as well as the carbon dioxide, so the reaction has only partly taken place, and at all stages it is a reduction rather than oxidation. In that case I cannot see where any oxidation can take place. He goes on to state in one place that carbon is changed in chilled form. I will read a short extract: Chilling of cast

iron is caused by the rapid solidification of molten metal, preventing the chemically combined carbon which exists in it as such from changing to graphite and thus giving a granular structure to solidified iron. He intimates further on that there is a kind of carbon in the molten iron which is not chemically combined. I remember some months ago we conducted several experiments on that point, and proved, to our own satisfaction at least, that free carbide did exist in molten iron and would exist even in a casting to a greater or less extent. When he says that the carbon is changed from the graphite or uncombined state, whatever it may be, into the chemically combined state, it is turned into a hardening carbon—that is what I understood—I wish to ask if he means the carbon is changed into an iron carbide of the formula which is properly most commonly taken Fe_3C , or that it is changed into what has lately been discovered and denominated as martensite. If we take into consideration the work that has lately been done in microscopy, we will see that the efforts of the last few months will throw a valuable light on this paper, and especially on the last part where he is discussing the changes which take place in chilled plates. At the same time from the fact that the retardation point is so very low, being about 600 degrees, the carbon actually changed to martensite can be a very small part of that which actually exists. It may be that a very small portion of martensite, as with titanium in iron, will so affect it that it will require only a very small figure of percentage to produce an effect on the ultimate strength or hardness of the metal. I would like to hear from Mr. West, to know if he has made any determination on that point.

W. N. Moore: There is one thing which Mr. West cites and that is the increasing graphite in remelting, I would say as mere corroborative evidence that we have found to our surprise that to be the case. We had supposed until the evidence was accidentally brought up that the more we remelted iron, the more we burned the carbon out of it.

Mr. West: Mr. Knight suggests several points that it is well to discuss. In the case of the melting of hard iron, cited by Mr.

Bell, more fuel is required in order to bring the metal up to a high state of fluidity. Mr. Knight gives that as evidence that soft iron will melt faster than hard iron. Foundrymen that are used to working with white iron or chilled iron know that it loses life very rapidly; and foundrymen of experience can tell by the eye whether or not the metal they have in any two ladles are about equal in their fluidity and temperature. If we have a ladle of hard iron side by side with a ladle of soft iron, which to the eye look equal in temperature, we know by experience that the white iron will very soon lose its fluidity. The soft iron may retain its life at times in a big ladle an hour longer, so to speak, than hard iron would. It is no reason because hard iron solidifies quicker than soft iron that the soft iron has a lower fusing point. The other question about silicon was with reference to my conclusion that the longer iron remains in contact with the fuel in the cupola and gases, the more it reduced its silicon. Now, these experiments and analysis which we have in this table show plainly that when we contrast them with the time of melting, there is far less silicon reduced where we melt iron quickly than slowly; and I think all experiments in the future will bear that out. The analysis in Mr. Knight's paper of yesterday show that the first large tap of iron is higher in silicon than that which follows after, and I stated yesterday that was due to the fact that the iron melted quicker. I know when we start, after we melt the first ten hundred pounds or so, that our iron melts faster than it does any time after, and that is due to the fact that a cupola is then clean; there are no chillings around the tuyeres; the openings for the blast are free; there is very little slag in the cupola, and all conditions are favorable to bring the iron down fast, so when it gets down to the melting point it don't take it as long to get an equal body to the fluid state as it does further along in the heat.

A vote of thanks was extended to Mr. West for his able paper.

Secretary E. P. Wilson, of the National Association of Manufacturers, made a brief address on the scope of the work of that organization.

The members and visitors then adjourned to the front of the Grand Hotel, where a group picture was taken.

At 2 p. m. the members and others in attendance at the meeting took special trolley cars from the front of the Grand Hotel and were shown through the suburbs, stopping according to previous invitation to inspect the plant of S. Obermayer Co., where a collation was served, the party returning about 6 p. m.

Upon the invitation of the local Committee of Arrangements, the visitors dined with the Cincinnati foundrymen at 8 p. m., at the Grand Hotel.

WEDNESDAY MORNING SESSION.

W. N. Moore called the meeting to order and presided, owing to the temporary absence of the president.

Mr. Sorge: At the banquet yesterday evening remarks were made by several of the gentlemen present as to the desirability of having the Cincinnati Foundrymen unite with the Western Foundrymen's Association. In a general way a plan has been outlined to the effect that it would be desirable to have as large a membership as we can obtain in Cincinnati and vicinity, and then to start a sub-headquarters at Cincinnati, at which meetings could be held on the same dates as the regular meetings in Chicago, which are held on the third Wednesday of each month. All papers that are to come before the Western Foundrymen's Association would be forwarded to the sub-headquarters, so that they could be read at the same time that they are being read in Chicago and be discussed here as well as there. The discussions of the two branches would then be put together in the joint proceedings and distributed to the membership. In order to facilitate this matter, and as we will probably not have another opportunity of considering it at this meeting, I want to say that there are a number of application blanks here, and we should like very much to have as many of you gentlemen as possible give us your application for membership, so that this movement can be started.

In the unavoidable absence of D. H. Truesdale the secretary read his paper on "Molding Sand." A number of very interesting castings were exhibited by Clinton A. Truesdale, illustrating the points made in the paper.

MOLDING SAND.

By D. H. TRUESDALE.

In these progressive days when there is a constantly growing demand for castings of a better quality, and foundrymen are striving in every way to make better work, and in many cases failing to produce satisfactory results, it seems proper to call your attention to what I believe to be one of the most prolific sources of failure.

Molding sand being the most important factor in the production of good work, it follows that it is of the greatest importance that the utmost care should be taken in selecting sand that is the most suitable for the work to be made. At the same time it is important to obtain a more thorough knowledge of the elements of which the best sand is composed.

The first thing to be taken into consideration is what are the necessary elements of molding sand, and in what proportions should they be combined. There are four qualifications that are absolutely necessary; these are, refractory qualities, porosity, fineness and a sufficient bond. Let us take these up in the order named.

First, refractoriness: The element which possesses this quality in the greatest degree is silica. Silica is found to be a component part in all the sand that is furnished to the foundrymen, but often in such small proportions that the sand is absolutely worthless. Being practically non-fusible, it follows that sand containing the largest percentage of silica that it is possible to work will give the best results. Only sufficient alumina should be present to form a perfect bond in connection with the water necessary to make the bond complete.

My experience in the foundry business having been altogether in the line of light castings, such as stove plate, hollow ware, and work of a similar character, I shall confine myself to what I believe to be the best sand for work of that class.

I take it to be self-evident that the only properties necessary in molding sand are silica and alumina. The presence of lime, magnesia, metallic oxide, etc., are detrimental. Of course I know that it is practically impossible to get sand with

just the proper amount of silica and alumina in combination. To remedy this trouble, I have found the best way is to make the mixture yourself. To do this in a practical manner all that is necessary is to get the pure silica, or as nearly so as possible, and it can be got up to 95 per cent pure, and mix with a fine strong sand containing at least 60 to 70 per cent alumina. It is impossible to give the exact proportions it would require in each case owing to the variation in the sands that are found in different parts of the country. Theoretically, the proper proportion of silica is 90 per cent to 10 per cent of alumina, but it will be readily seen that these proportions are, in the every-day hurry and push of the modern foundry, very difficult to keep up, as the clay in the strong, fine sand, vitrifies and adheres to the castings each day, so that the proportions would be changed after each heat.

I have found from practical experience that from 33 to 40 per cent of silica, with such sand as I spoke of before, gives the best results, making castings with a smooth surface and sharp lines, and also, what in my estimation is of greater importance, enabling the foundryman to dispense to a very large degree with the use of foundry facings.

There are on exhibition here some specimens of "art" castings; also some ordinary stove plate made in the manner spoken of, that will speak for themselves, which you are invited to examine.

Second, porosity: It is hardly necessary to say much about this, as everyone understands its importance. It will be found that by the use of silica, owing to the crystal formation of the particles, this property will be preserved to a degree that will in a great many cases render venting unnecessary. I have seen large and intricate castings that have been considered hard to make, owing to the difficulty in venting them, made with complete success without the use of a vent-wire, which result was brought about solely by the use of silica sand.

Third, fineness: This is a matter for which there can be no rule, as the term fine sand is a very indefinite one. What one foundryman terms fine sand another would pronounce coarse;

so the question of a proper degree of fineness must be left to individual judgment, in accordance with the work to be made. I wish to say, however, that in my opinion a proper use of silica will enable the foundryman to use sand that is much finer than is commonly supposed possible on medium heavy work, thereby making castings with a better surface than with coarse sand.

Fourth, bond: By this is understood that constituent which joins and holds the particles of silica together during the process of pouring. For this alumina or clay is found to be the best, as silica, having absolutely no adhesive powers by itself, must have some material possessing the adhesive or "sticky" property necessary to hold the particles together while the mold is being filled. No more alumina or clay should be present than is necessary, as any increase over the proper percentage makes bad, rough work, by its melting and adhering to the castings, causing extra expense in cleaning, and at the same time incurring the liability of loss; in many instances causing blow-holes owing to the increased density of the mold, thus preventing the rapid escape of the gases.

As we have to use water to complete the bond and depend upon it to keep the mold intact, while being poured, it follows as a matter of course that the sand that will stand the use of the greatest amount of water is the best sand. In all cases as much water should be used as possible and at the same time prevent "blowing."

A judicious use of more silica in the sand will convince any foundryman of its value, and once having proven it, no backward step is likely to be taken.

The ideas presented in this paper are solely the result of several years' experience in the use of silica sand, after having proven to my complete satisfaction its value in the production of good castings.

There is another feature in connection with silica that I have not spoken of, but it is well worth considering, that is its use in making cores. It will be found on trial, that cores made with it alone and oil, will result in smoother, cleaner castings than any

other core sand, at the same time dispensing entirely with "core wash" of any kind, as the refractory powers of silica absolutely prevent any burning of the sand, causing the core to fall out at the tap of the hammer, leaving a very smooth, clean surface, better than I ever saw by the ordinary way of making cores. Also the sand can be used over again indefinitely, as it is in no way injured or changed by having iron poured on it, except to be discolored by the burning of the oil, which does not alter its value. I have tried this repeatedly and know from experience what I say is true. A trial will convince any one of the fact.

I cannot but believe that a more thorough study of the value of silica in molding sand will result in what all engaged in the business must admit is greatly to be desired, that is, the production of better and smoother castings, resulting in economy in cleaning, finishing, etc., as well as gratifying the pride of every true foundryman.

Charles A. Bauer: In our practice no facing is used. Our castings are about of medium weight, and we get a very fair surface without facings. The sand that we use is known as Sandusky sand. We use for our cores almost exclusively silica sand, with very good results.

C. F. McGilvery: Some years ago we used some silica sand. I think it is called Dresden sand. I ordered a car of the sand and though we would do without the facings, we had trouble with the molders to get them to use it. They were not careful enough. Since that time we have had to use facings. There is no doubt that if you use sand with fine silica you can do without facings and I think make better and sharper castings, but that sand has not got the bond that the ordinary molding sand has. It requires harder ramming and a little more work, and the men object to that. Still I think it makes a better surface on the castings and makes them sharper, possibly because you have to give them a harder ramming. As a rule too much fine sand is used and too little elbow grease. I think there is one of our mistakes in rough castings. You take fine sand and ram it hard and of course it will stand. To make the surface the same on

molds it is necessary to ram coarser sand a little harder. That requires more labor, and that means more money.

John D. Sadlier: I, too, have had some little experience with facings and sands of different character, though rather for use with a larger and heavier grade of castings—loam work and dry sand. I believe that I was one of the first users of silica sand as a facing for molding in the country. That was brought about some years ago, when steel was introduced to this country as Bessemer steel. I was employed by the Cambria Iron Company, at Johnstown, for about three years, from 1871 to 1874, in experimenting to get a facing that would stand the steel. We found after a long experiment that silica sand was about the only sand that would give satisfaction. It required a very fine grade of sand, as in the steel the pores or crystals or fibres are very fine. In order to prevent them from penetrating, permeating and entering into the pores, you have to have it finer than the crystals of the steel. It requires a sand without a great expansion, so that it would stand up against the intense heat at which steel castings are poured. So we found that silica was the best sand to stand that. You can grind it much finer and have an open and porous sand that would prevent the steel from running in it. I found afterwards that the use of silica sand in loam work was a great advantage; taking burned sand, burned silica, or burned core sand—and using a quantity of it in the loam you get a mixture from which you get a wonderful result. I have a simple method for testing sand where you don't have a chance to make chemical analysis, which anyone can use—make up a ball of sand and subject it to a severe fire, similar to what you get when the iron is poured against it, and if the sand browns nicely instead of crumbling away, you can almost be sure that it will stand the iron. We had an instance of that some years ago in making some very large gears. We had trouble getting the teeth. The scales would fall off the mold and you could just brush the teeth right off. I was called to see if I could find out where was the trouble. They had a mold made with the teeth all around under the gear. I took a quantity of waste, saturated it with oil and ran a ring round under near the teeth. There

were no arms to interfere, and there was a good chance to put a ring in front of the teeth. Then I lit a match to it and you would see the teeth slide right off. There was some kind of soft quicksand of clayish nature, and the moment the heat got against it, it didn't seem to dry in the bond; it seemed to slide away. If you take a ball of sand and subject it to the fire and it stands up against a quick fire, you may be sure it will stand the iron, if it is open enough so it will allow the vent to escape. It is a simple, easy test. Anyone can try it and keep governed as to the fineness, according to the work required, where it is dry sand work. In lighter or finer castings you have to be governed by the fineness of the sand to have a nice surface on the casting, and, as I have said, you can determine in a moment by subjecting a ball of sand to a quick fire whether your sand will stand in the mold. If it does that, you can be pretty sure that there is some other reason for not getting good castings.

Mr. Moore: Your experience has been in the nature of bringing up sand which contained a larger amount of bond and a small amount of silica.

Mr. Sadlier: I find that causes more trouble than anything else. Sand certainly requires a certain amount of bond. As much water should be used as possible. Some make a mistake in having sand that is really too close, too strong, and with too much clay. You must use water enough and ram it hard enough. These things usually cause the loss of more castings than anything else I know of.

H. F. Frohman: I think the gentlemen here have a wrong idea in regard to what we term "facings." We term facings any material which is refractory, that is, which will stand any amount of heat greater than melted iron. The general accepted idea is that any thing black is a facing. That is a wrong idea. During the last few years we have been introducing a silica, also used for facing, which stands a greater amount of heat than some of the other black facings, especially on such work as steel, where the iron is poured with a great amount of heat. Regarding silica, if you grind it fine enough it will make a good facing. Silica

sand itself will make a good facing if fine enough; but my own idea of a facing is to overcome that defect in sand of which Mr. Truesdale says: "No more alumina or clay should be present than is necessary, as any increase over the proper percentage makes bad, rough work, by its melting and adhering to the casting, causing extra expense in cleaning." As you know, in all parts of the country they have not got the best sand, and it costs too much to bring the best sand to those points, where they might otherwise be used to advantage. Many have not the facilities, nor do they care to invest in sands most suitable to their class of work. We recommend and it is absolutely necessary in such cases to have facings to cover up and protect that part of the sand from melting and adhering to the casting. In regard to the non-use of blackings and facings on certain kind of work, we have seen—this not my experience only, but it is a fact—that can be done, although looking at it in a business point of view, it might not be the thing for me to say that castings can be produced in that way; but we always claim there is extra expense in so doing—that is, probably more so than with the use of blackings and facings.

C. S. Bell: In our line of business we have tried almost everything. I suppose you are most of you familiar with the sand from Sandusky. We have found the sand, which is recommended as suitable to make castings without facings too expensive, costing in some instances as much as \$4 a ton, when, on the other hand, we could buy sand which was satisfactory to us at from \$1 to \$1.25. This sand we could not find anything the matter with, consequently, we have abandoned the higher-priced sand, some of which has a national reputation, and are using a sand which we can get for about five cents per barrel, which we consider a very good sand for our purposes.

Clinton Truesdale: In bringing forth my little explanation of castings this morning (referring to castings on exhibition, including bust of ex-President Harrison), I did it for the purpose of carrying out the desires and wishes of my uncle, D. H. Truesdale. I wish to say that I have been associated with him and got

my training strictly under his observation for the last twenty years; and I say without any fear, and I know some of the gentlemen will bear me out, that I consider him one of the brightest men in modern foundry practice. These castings which I have been showing you are strictly in the line of stove-plate, light castings, and Mr. Truesdale's experience and mine also is confined exclusively to stove shops.* In fact, I have no knowledge very far reaching in respect to heavy machinery, or anything of that kind; but we have been giving the subject of silica considerable study for some time, and how well we have succeeded I leave to the gentlemen here, and after they have examined the display I think they will bear me out that we have got along very well. In regard to the facing question, I have succeeded in the past two years in grinding silica sand in ball mills for facings, thereby dispensing with the use of facings. In saying this I am not trying to keep my neighbors from going to the Obermayer Company. I introduce from 33 to 70 per cent of silica sand, according to the grade of work. I conceived the idea of trying to grind it and use it exclusively for facings. I had two ball mills placed in the shop for the purpose of grinding our facings. After awhile I succeeded in introducing it altogether as a facing. I have used a proportion of three-fourths silica sand and one-fourth Newport sand, found over in Kentucky, our neighboring state. I also found it gave good results in adding to it fire clay when made into cores. I have succeeded in introducing silica sand, a very fine grade, found sixty miles south of Louisville, in the foundries here in the city, where they have never had opportunities to make large cores—among them Erhart & Co. They tell me they have had great results, where heretofore they have lost three or four out of five pieces of casting caused by a bad core, which difficulty they have now overcome. They have dispensed in a great measure with their blacking. Cores made of silica sand will run out, as D. H. Truesdale says, at the mere tap of a hammer. Castings made with silica sand will not scale as readily as those made with facings, and have a very fine smooth surface.

I have on exhibition this morning some castings showing gates directly on the face, very fine lines; also some gated on a smooth surface. You will see at a glance how it scales and cleans. It is not vitrified or burned off. In fact, in my judgment, there is no end to the use of silica in modern foundry practice.

H. S. Vrooman: All sand contains more or less silica. I can't see wherein silica introduced into sand could make it peel from the casting any more freely. There is only one point—it makes it more porous. If you take the iron, for example, that contains all the carbon up to a point of saturation, you could introduce no more carbon into it and make it hold. If you have a sand that contains a sufficient quantity of silica, the introduction of any more will not do any good. As far as making it more refractory, I can't see how it will do that, either; because the alumina in itself is non-fusible—the silica is non-fusible. I can see, as this gentleman says, a test in trying sand to see whether it will hold up under sudden heat. There is only one thing which that shows, that the sand which comes down very quickly under sudden heat contains a large amount of vegetable matter. The introduction of silica sand will possibly make that percentage of vegetable matter so much smaller in proportion to the amount of sand that it will not show, and, therefore, not come down. And if the sand contains a sufficient amount of silica in the beginning, I don't see wherein this introduction of silica will do any good. If we have to hunt around to find sand that has 60 to 75 per cent of alumina, in order to carry the silica, you could just as well look around for a sand that has a sufficient amount of silica in the beginning without adding it. Now, silica is in two forms, combined and free, in sand. All the sands we have contain a certain per cent of silica. The variation in the percentage may affect its porosity or refractoriness. The question as to how long sand will wear or stand heat does not depend, in my opinion, upon the amount of silica it contains, nor on the amount of alumina it contains. On the other hand, it depends upon the crystal formation of the sand. Some sands, or crystals, are conductors of heat. Other crystals are not conductors of heat. Chemistry tells us that alumina, when subjected to red heat, loses its adhesive

qualities. So if the sand has been subjected to a red heat, it will no longer retain the water. You may take a fire-clay, supposed to be as refractory as any, make a brick of it and put it in the oven and subject it to a red heat, then take it out and regrind it, and it will never combine with water again; the adhesive quality is gone, because it has been subjected to a red heat. In the sand it is the same, because wherever the face of the mold comes in contact with the molten iron, the face of the mold must become heated to a red heat; and owing to the structure of the crystals, whether it is a conductor or a non-conductor of heat, that red heat penetrates further into your mold. The lines are well defined. If you shake out the mold and take the casting out, you will find a dark colored line. Seems as though the sand has dried out. In some instances it follows deeper into the mold than in others. If you get a sand crystal formation that is a non-conductor of heat, then red heat penetrates further into the mold, owing to its crystalline construction. As to the amount of alumina or the amount of silica which a sand contains, I would advise all to use as open sand as it is possible for them to get, for three reasons: First, that you work it better; second, you can ram it harder, and third, it will vent easier. There is no question that a sand suitable for any class of work can be found in nature, if the consumer wants to pay for it; and a sand that is strong of clay does not make a long-lived sand. Mr. Sadler can take a sand very high in clay or alumina and test it with his sudden heat and it will stand; at the same time it will not make a good sand for the work.

John D. Sadler: In reply to the last remark, I would say, that I grant that clay can be subjected to severe heat and still stand it well; but I would suggest that at the same time the sand would have to be judged as to whether it was open and porous enough, in order to allow the vent to escape, and if it did it would stand up to the work and the iron—if it stood the test of a severe fire and the test was similar to the iron running into the mold, something similar to iron heating up the surface of the mold very rapidly. If it doesn't stand a severe fire similar to that, it will not stand in practice. I suggested a simple test. I have found some

sands that won't do in the molds; they will crumble away. With reference to the use of Sandusky sand, Sandusky county is a wonderful county for limestone. Nearly all Sandusky sand has a great percentage of lime. Some of the sands we use from Sandusky have got more limestone than finer sands, which burn out and forms a quick lime by the heat coming in contact with the limestone in the sand. This will mix in with the iron of the casting, and sometimes produce holes in it, especially if the sand is worked just a little dry. Right in front of the gate, by the heavy body of metal running against it, it will burn, the same as limestone will burn in the kiln. It will burn back for some little distance and give some trouble.

Mr. Moore: Mr. Sadlier, your use of silica has been in bringing up the silica in sands which were deficient. It would be interesting, I think, to those present to know the percentage which you found necessary.

John D. Sadlier: Well, that I would be unable to give, because I have never had an analysis made of any of the sand we use. But that can be very readily determined as to what point you desire to bring it up by actual experience in the shop. You can see just what amount would produce the best results in any one particular case. In one instance, a firm in Cleveland was having a great deal of trouble with their loam castings; they would be covered with small light scabs, literally covering the whole surface of the casting. They asked me to look at the work. It was an awful job; these light scabs were not more than one-tenth to one-eighth of an inch in thickness. I took some of the burnt sand, which had a good deal of silica in it, and added it to the loam to open it up, and that ended the trouble there. A great many foundrymen throw out just what they want when they throw out sand that has once been used, under the impression that sand that is burned—as in the case of cores, etc.—is waste; but a great deal of it is the best thing that foundrymen can have by careful, judicious use of it. Too much of it is not good; but a certain proportion or quantity of it, and in regard to the proportion, you must be governed entirely by the sand which you have. In a new sand you will have to be guided by results from actual experiment in practice.

Charles A. Bauer: I wish to state that we take from 8,000 to 10,000 tons per year of the Sandusky sand, and of all the sands we have yet used have not found any, without exception, that was as free from lime as the Sandusky sand; and I would say that there is only one sand that I have ever known which contains less lime than the Sandusky sand. I want further to say that any one who has ever analyzed any of the Sandusky sand will find that lime is an awful scarce quantity in it.

H. S. Vrooman: I think the presence of iron in sand is evident from this reason, that you take a sand red in color, say Sandusky or Zanesville sand, it is evident that it has considerable iron. That you will always find to be true in sand that is red in color. It seems to me as though this iron disintegrates in some manner. When such sand lies out in the open air there is a kind of oxidation takes place in that sand and it becomes soft and free, whereas, sand which does not contain considerable iron, under the same circumstances becomes hard and lumpy and you have got to crack it with a hammer. In regard to fine sand, I think they carry it to an extreme, try to make too large work with fine sand. Take a sand that is too fine, when you take your pattern from your mold and let it dry, you will find on the surface a very fine quality of sand that adheres to it. If the sand is coarser that will not occur. The sand is too fine in my opinion.

Mr. Moore: I visited a little foundry down in Birmingham, Alabama. It was a very small foundry—very crude. They were making grate bars, manufacturing castings which were good enough, but very rough. I was very much surprised, for from the character of the foundry and the help employed and their lack of ordinary appliances, to note the excellence of their castings. The sand which they use there is of a reddish nature, and presumably because there is a great abundance of iron ore in the country, contains considerable iron. The value of iron in a sand mixture—the question of its value occurred to me then, and I have been very much interested, and hope that some one is able to cast further light upon the question which Mr. Vrooman has raised.

S. S. Knight: I suppose in the last year we have made some two or three hundred analyses of various molding sands throughout the country, from Albany molding sand to as far west as Denver, Colorado, and in the whole catalogue we have scarcely found enough lime in any one molding sand to amount to anything at all, that is, in the sand itself. It is very seldom that lime is found in sand, and when it is there, if the sand has ever been fused, it is in the form of calcium silicate, because calcium being a very active agent, it will naturally combine with silica di-oxide, or with alumina. There is one thing in which all foundrymen are interested, and that is, some standard grading of sand. It is now purely a relative matter, nothing absolute about it. If the standard is to be based upon analysis, the ordinary silica, we hold, should run at least 70 per cent, the iron oxide should not be below 6 per cent, and the alumina, or, rather, more strictly speaking, alumina itself should be at least 4 or 5 per cent in addition to what would naturally be confined to the silica. However, the best test which I know for the fineness of sand is simply weighing a certain quantity of your sand, drying it first in the oven at a temperature above the boiling point of water, 212 to 220 degrees Fahrenheit, and putting it through a standard mesh sieve, say 30 meshes to the inch. Of course if you want 40 meshes to the inch, it can be used. The test is purely relative; but whatever sand you are using you can afford some idea to anybody as to how much of it you can force through a certain sized sieve by shaking it for a definite time. I say in regard to the activity of iron oxide in a sand, a molding sand that would come to us without at least 5 per cent of oxide in it would be rejected. The iron oxide in a sand simply gives it a red color, of course, but it gives it a certain strength and tenacity which nothing else does give it; and if you are using facing on your molds, the one thing to be watched, as far as we know at present, is clay, because clay is nothing but decomposed feldspar. When you have a great deal of clay in your molding sand, which is largely silicon di-oxide, the ash of your facing will fuse up with it and you are going to have a scale on the casting.

Mr. Moore: You state, if I understand you, that you don't consider a sand which contains less than 6 or 7 per cent of iron oxide as proper, at least for your purposes. Do I so understand you?

Mr. Knight: I made that statement. However, we have a great deal of sand that runs nearer 5 per cent, but nothing below 5 per cent will be accepted. Of course, you understand our work is not ordinary work. Possibly you could go as low as 3 per cent for ordinary work; but I can't see how any sand with less than 3 per cent would be accepted.

Mr. Moore: If there were 10 or 15 per cent of iron oxide in the sand, what would be the result?

Mr. Knight: In reply to that, I can only state that I can scarcely see how you could have 15 per cent in your sand, unless you are crushing iron ore for the purpose of putting it into the sand. In that case it would be possible, if you ground up your ore and put it in the sand. However, your sand would not be increased in strength, because you are simply getting a physical mixture. In molding sand you have iron disseminated through the entire mass in a way you could not possibly do physically, if you were to put it in as a disintegrater.

Mr. Moore: The iron which you find in sand is in what form?

Mr. Knight: In the form of an oxide.

Mr. Moore: And associated with the alumina, or with the sharp sand, silica?

Mr. Knight: I suppose you mean whether it is a chemical combination or not, with the alumina or silica? I have never made any analysis in regard to that point. I hardly see how it would be possible for it to be associated with alumina or silica either. It would be in the form of iron oxide. It might be possible to have some such combination as alumina ferride or ferrate.

Mr. Vrooman: I agree with Mr. Knight in what he says as to the necessity for a better grading of sand. At present nobody grades sand alike. A man strikes sand on his farm, possibly in

two veins, and he brings it in and says one is No. 1 and the other No. 2. He doesn't know why, nor nobody else; he sells it to us. Another man comes along and he says, "I see you are using No. 2 sand." He has struck something that he calls No. 2. It may be finer or coarser than the other man's sand of the same number. The way to grade sand is to put it through meshes. I would say with sand that 90 per cent of it goes through, I would call it by the number of the mesh that you had forced it through after being dried. Then if you bought sand from anybody and found it to be too coarse or too fine by that standard, you could so designate it; you could tell the man it was too coarse or too fine, and he would know how to furnish it to you.

Mr. Sadlier: I would like to ask a question. I never had an analysis made of sand in my life; but there is an appearance, right along where there is a gate—where the metal strikes the mold, of a white substance, and afterwards you can find small portions of a white powder that resembles lime, back in the sand. I ask, if it is not lime, what is it? I haven't had any possible guarantee that it was lime, but our sand came from a locality where there are large quantities of limestone and I reasoned that the limestone, or rather a small portion of it, was taken up with the sand. I would like to ask what that white powder is that appears on the surface of the mold along the casting, that sticks to the surface of the casting, and if you break it you find a white substance resembling lime?

S. S. Knight: I don't understand the question the member asks. I have never seen that occurrence in any foundry practice. If he will send me a sample of that substance, I will tell him what it is if I can.

H. S. Vrooman: I think that is lime, showing the depth to which the red heat has penetrated the sand, and possibly there may be some vegetable matter in it; no doubt, but an ash there possibly resembling lime.

Mr. Sorge: Mr. Knight said he never in any case found a sufficient amount of lime in any sand to do any harm. Will you let

me know what percentage you did find in it, and whether you didn't occasionally find carbonate as well as sulphate?

Mr. Knight: In reply to that, any carbonate in molding sand would be in such slight quantity it would not have a tendency to fuse the sand. I couldn't say as to the percentage. I would have to look back over our books. I am sure it was below 1 per cent.

Mr. Sorge: I know there is sand in our direction which contains between 4 per cent and 5 per cent of carbonate. In the locality of Joliet, I know the effect of the lime is very distinct and disastrous there.

Mr. Knight: I can see that if lime comes up to 5 per cent it would be disastrous in coming in contact with hot iron. It is beyond our experience to find any sand of that character.

Mr. Sorge: Joliet sand is what I referred to.

S. S. Knight: I am glad to know such is the case, if there is such a sand; because it was entirely beyond my knowledge that such sand existed.

Charles A. Bauer: I should think if any molding sand contains 5 per cent carbonate it ought to be ruled out and called by some other name. It has been baptized improperly.

Mr. Sorge: It was ruled out. It was called molding sand for a great many years.

Mr. Vrooman: When I was working at the trade, it was very often the custom—where we had a flat surface—to shake some lime on the face of the mold, which seemed to keep the sand from cutting, and also held the facing better. There is a simple test that can be made by any body to see if lime is present and will not do any harm—take some muriatic acid and drop it on your sand; if it boils up there is lime in it.

Mr. Sorge: That won't show the sulphate.

Mr. Sadlier: In relation to putting lime on the mold, the trouble comes in at that point where being subjected to heat the change is going on from stone to lime. I have used lime and

used lots of it in connection with the reduction of metals. Perhaps on the mold it is all right, it creates a moisture and holds the sand together; and oftimes prevents washing. I don't know of anything really much better than a little lime dust used the same as it would be in facing.

The Chair: Since we have been in Cincinnati, as we have expressed individually and on several occasions, we have been entertained in a manner which is beyond all criticism; more than that, which is entitled to all our thanks and praise. We have highly appreciated the efficiency with which it has been done, and the care with which our wants have been supplied and our enjoyment administered to. We have, however, not as yet put upon the record of the association any formal expression and it is desirable that this be done. I wish some gentleman would prepare a motion to that effect.

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S. T. Johnston: We have all had a very good time. Our entertainment has been lavish, and nothing gives me greater pleasure than to move that this association spread on its minutes a resolution of hearty thanks to the Cincinnati Committee and those connected with them who have provided for our entertainment, and manifested to us such great kindness and attention.

Being duly seconded, the motion was carried unanimously.

A vote of thanks was extended to D. H. Truesdale for his paper on "Molding Sand."

The announcement was then made by Henry Gossiger of the death of Chris Erhart, one of the old foundrymen of Cincinnati, which had occurred during the meeting.

On motion the association extended to the family and business associates of Mr. Erhart the sympathy of the association in their loss.

At 1 p. m. those attending the meeting, to the number of about 100, boarded the special cars furnished free of charge by the C., H. & D. R. R. Co. and proceeded to Hamilton, Ohio, under the guidance of Col. R. C. McKinney. After being photographed in a group in front of the office of the Niles Tool Works, the visitors

were conducted into the office, where a bountiful and choice lunch was provided for them, of which they partook, washing the solids down with plentiful draughts of sparkling champagne, and being entertained by speeches from various gentlemen. They then proceeded to inspect these famous works and were fully occupied until nearly 5 p. m., when they returned to Cincinnati in the special cars, after having expressed their appreciation of the officers of the Niles Tool Works and the excellence of the plant by raising cheers and votes of thanks.

A REVIEW OF THE FOUNDRY LITERATURE OF THE MONTH.

AMERICAN MACHINIST.

In the issue of October 7 a representative of this journal describes some observations made on a visit to the Worthington foundry at Elizabethport, N. J. We reproduce the following abstract from the article:

"The stock of patterns is, of course, a very large one, and a separate and isolated house is provided for their storage, with a complete record by division, section and shelf, and pattern designation and number of the location of every pattern. Five men are constantly employed in the storage and conveyance of the patterns to and from the various foundry departments. The order for any casting, or any lot of castings, has attached to it a series of coupons, which are successively torn off, dated and signed, and turned into the office as the different steps are accomplished in the progress of the order through the works. The office records will thus show, and the superintendent can instantly see, at any time when the pattern was delivered to the foundry, when the cores were delivered, when the molder got the order, and so on.

Many patterns are changed at different times and for various reasons. The record of these changes is kept in the drafting room. When the original designation number of a pattern is once established, the changes on it are designated by successive letters of the alphabet. If a pattern was originally "M. K. 17932," then "M. K. 17932A" would indicate the first change, and "M. K. 17932H" would indicate the eighth change which had been made upon it. The marks actually upon a pattern at any time always indicate what it is at that time, whether changed or not. We saw a pattern just going to the foundry with the "H" change on it to fill some repair order.

In this establishment electricity and compressed air work side by side, each employed at what it can do best. Electricity

lights the establishment and drives the traveling and other heavy cranes where the larger work is made. Compressed air is also carried all through the works for vertical direct-acting hoists wherever they are required, and also for the numerous molding machines employed. There are two engines driving the dynamos and engines for the three fan blowers for the three cupolas, which, we are assured, are a relic of the Dark Ages, and their presence is a reproach to any modern plant. They give absolutely no guarantee as to the volume of air delivered. It all depends upon the freedom of the tuyeres and air passages, and if these become choked, as they often do, the fan blower tamely accepts the situation, while a positive pressure blower would insist upon its rights every time. Besides all these, there are one or two water pumps, although, rather strangely, water is not as much in evidence in this establishment as might be expected.

Of the foundry as a whole, the distinguishing feature, as compared with the average foundry, is in general use of the molding machine. It is here used for practically all the standard work of the concern, and up to sizes weighing more than half a ton each. The molding machine is here probably to be seen at its best, both as to opportunity and as to performance. While it is at once a great saver in labor cost, and while it secures more excellent and uniform work than is possible with unaided hand labor, the cost of installation must deter any small concern from going into the system very deeply.

For small work, such as, for instance, much of the bronze or composition castings, hand-operated machines, designed and made at the works, are employed. In these the sand is rammed or pressed by a pull on a simple lever. One or two Tabor machines are also in use, but the chief machine employed is the Indianapolis machine, whose general features are sufficiently well known to the trade. These machines are employed on work of considerable size and weight, so that the flasks are handled by pneumatic hoists which come right over the flask on the machine, pick it up, run it off by an overhead trolley and set it down on the floor. The machines are duplex, and make cope and nowel alternately. For each machine a circular pit is built, and the

machine is sunk sufficiently to bring the top of the flask to a comfortable height for the men. One half of a flask will be set on the pattern plate, the sand shoveled in and the pressure applied; this being equalized by rubber bags filled with water. Then the sand is struck off, the pattern is drawn down out of the mold by the movement of a lever, the same movement throwing up the pattern in the opposite plate. The flask is lifted off by the air hoist and run off to the floor. The other plate with the other half of the pattern is then brought around, and the operation goes on. These molds, before closing, have usually a lot of intricate cores set in them. Each machine is usually operated by three men; two at the machine itself, and the other at the hoisting and conveying; and we were shown double-pump cylinders of which the three men would turn out thirty per day. The machines of this class employed here, with the immense stock of carefully fitted metal patterns which are stored in an isolated house of their own, may have cost nearly \$100,000, so that such a system is not for everyone to think of adopting.

THE FOUNDRY.

W. J. Keep in his monthly contribution to "Cast Iron Notes" writes on the subject of welding cast iron to steel, wherein he says:

A couple of years ago I happened to go into a foundry where they were making rolls for mills to be used in grinding sugar cane. The rolls were molded end up. Just before pouring, the mold was opened and a warmed steel shaft was set in prepared recesses in the bottom and top of the mold and melted cast iron was poured around it. Sometimes the iron became apparently a part of the steel, while at other times the iron would shrink away and leave the shaft loose, in which case the iron had to be broken off, as the roll was of no use. The steel shaft was warmed to prevent its condensing the moisture of the mold, and its surface was freshly ground to cause an unoxidized surface to be presented to the inflowing metal.

The method employed in Philadelphia, in welding rail joints, is to scrape the rails clean and bright, and to dry out all moisture before the cast iron is poured around them.

About the same time a founder from the far west had taken a contract to make some castings, in each of which was a small steel shaft. In the sample casting the iron and steel seemed to be welded, but in the castings which were made the steel was loose. Also about this time the superintendent of a mill where a large amount of steel rails for street railroads was being made wrote for information on this same subject. The crossings for these tracks are made by placing the rail ends together, with necessary steel surface pieces, and then pour cast iron around them. There were often failures in making a mold. Each of these people had tried every flux that they could think of. In casting steel along with cast iron I had covered the steel with silicate of soda. The silicate was diluted with water until quite fluid, and heated. The steel, previously brightened to remove oxide, and warmed, was dipped into the fluid silicate.

I suggested in each of the above cases the use of fluid silicate, applied to polished surfaces with a brush. For the rolls and for the railroad crossings it proved a perfect success. For the heavy railroad crossings, after applying the silicate and before it was dry, powdered ferro manganese was dusted on and adhered to the silicate. The silicate forms a coat of glass over the surface of the steel, which melts when the molten iron strikes it and flows ahead of the iron. This keeps all air and moisture from the joint, and if no oxygen can reach the surface there can be no rust. If the surfaces are bright and the iron hot, there will be an absolute union. Inch square steel bars having four inches of their ends bedded in the cast iron could be pulled out when no fluxes, or when other fluxes were used, but with the silicate and powdered ferro manganese the steel bars would break off and not pull out.

It was at first thought that peculiar cast iron was needed to prevent the steel from becoming loose, but the success in the use of silicate showed that if oxide did not form on the steel surface there would be a weld.

The same author replies to a question regarding the "Melting of Different Grades of Iron at one Heat." We append both question and answer in order that the reader may thoroughly understand the reply:

Please give us your opinion as to the advisability of melting several grades of iron in the same heat. For instance, in a 28-inch cupola, with tuyeres 15 inches above bottom, melting about one and one-half tons per hour, melt about one ton No. 3 pig iron for plow mold-boards, one-half ton for plow-points of No. 3 pig iron and good machinery scrap, then one ton of cheap scrap, such as stove plate, grate bars, old pots and burnt castings, for making a line of grate bars or other castings where it is not specially necessary to turn out first-class goods. Can these different grades or lots of iron be successfully melted and run into the various castings without getting the different grades mixed?

By placing a larger charge of fuel above each grade of iron than is necessary for melting, there will be no mixing of the different grades. Iron cannot melt until it reaches the melting point. The excess of fuel must burn away before the next iron can reach the melting zone of the cupola. By estimating the amount of iron drawn out, the founder can tell when any kind is melted, and when the iron stops coming he knows that it is all down and will stop the tap-hole until the extra fuel burns away and the next iron accumulates.

The hard iron should be charged first, because the cupola is not as hot as later, and because it is safer to get the harder grades out of the way. For small cupolas, coke is more likely to hang up. With anthracite coal iron can be melted in a cupola 16 inches diameter, or even smaller. With coal, the charges are more certain to descend as they are put in, but with a 24 or 28-inch cupola coke is all right.

The above description of the mixture of grate bars and all of the castings exclusive of mold-boards and plow points, suggests the question, What is the best mixture for grate bars? If a founder who makes a specialty of grate bars would experiment with mixtures and keep a record of the life of the bars while in use, the information would be valuable.

R. Moldenke in a paper dealing with the subject of "Test Bars" advises standardizing the methods of testing and suggests the propriety of this question being taken up by the American Foundrymen's Association. We reproduce the following abstracts from the paper:

Once the mode of preparing the test bars, and the shape they should be in to best represent the actual conditions in the delivered work, has been determined upon in this manner, the structural engineer may come in and decide how the bars should be tested to give him the best idea of the nature of the material he has to deal with.

This brings up the question of the purpose of test bars, and it would look as if the subject branches out here into several subdivisions. Primarily the test bar is to show its maker what information he can obtain in regard to the physical quality of the material he has cast. In other words his product is already complete, but he has to some extent a safeguard which will enable him to ship his work with an easy conscience, or scrap it if not up to requirements. Secondly, the test bar serves as a fund of information, the careful study of which is of great value to the founder in subsequent work. In both cases just mentioned the test bar should approach the leading dimensions of the work, or better still, be cut directly out of a casting rejected for some surface blemish. Even if test bars are made regularly, it will pay to compare the results obtained from them with those of bars cut out of castings, for experience with hundreds of bars so prepared contrasted with thousands of the regular make has shown the writer that the results are always in favor of the separately-cast bar.

C. Vickers furnishes an article on "Economy in the Brass Foundry," in which he advocates that more attention be paid to the small leaks usually found in establishments of this character.

IRON MOLDERS' JOURNAL.

A contributor, writing of skimming gates, says:

It stands to reason that where a first-class grade of iron and best quality of coke are used the probabilities of securing good,

clean castings are much better than where inferior material is supplied. In fact there is, with ordinary care on the part of the molder, very little difficulty in turning out clean work with good stock. But every man of any experience in the foundry is fully aware that the constant cheapening of prices and the constantly increasing keenness of competition in the cast iron trade has led to the use of inferior grades of both iron and fuel. Notwithstanding the inferior character of the material supplied, the demand upon the molder is ever for good, clean castings. Every reasonable man will admit the impossibility of making first-class castings out of very low-grade iron. Let the mold be as perfect an example of mechanical skill as could be constructed, the casting will partake in its composition, not of the excellence of the mold, but of the quality of the metal poured into it. The shape may be perfect and yet the casting worthless.

As a result of his efforts to meet the demands upon his skill the molder has resorted to various devices to make up in some degree for the inferiority of the material or the result of accident. Among these one of the most generally used and withal the simplest, is the skim-gate. And yet even that simple device is the result of much intelligent thought and observation.

The natural tendency of the impurities in molten iron is to rise to the surface, they are lighter than the iron itself, and, following a well-known law, they float like a cork on water. In certain irons and under certain temperatures, these impurities, or "slag," as they are commonly called, will be united and held together by a film or scum that forms on the surface, and it is then comparatively easy to pour a mold without any of this "slag" being introduced with the metal. When this scum does not form, a substitute slag-binder can be formed by throwing a handful of dry sand lightly over the surface of the metal. But in spite of precautions of this kind and the assistance of "skimmers," it has been found that dirt would get into the mold, and experience ultimately proved that the period of greatest danger from this source was with the first entry of the iron into the mold. The problem then is, how to intercept this obnoxious dirt and stow it away safely so that it will not have a chance to enter the mold proper.

The author illustrates the style of skimming gate preferred by him and makes further comments upon the utility of this device, when constructed upon correct principles.

R. D. Moore, discussing the "Pressure of Fluid Iron," writes:

All observing molders will have noticed that in thin plates poured very fast and provided with but one small riser, the metal will be forced up an inch or more above the top of the riser, then fall say three inches lower when the metal comes to rest. Had four large risers in the shape of wide sprues been set close the metal would have been raised very little above the standing height.

The figures in this case tell a very interesting story. Three inches extra rise of metal gives three-quarters of a pound of unnecessary pressure on every square inch of surface or 108 pounds on every square foot, and these figures don't tell the whole story of strain. A gun barrel is burst by a plug of snow in the muzzle. Why? Because the gun is burst before the powder had time to remove the plug. In like manner the strain upon the mold under a rapid pour is much greater than the rise of metal will show, that being also largely a question of time. The rapid fall of the metal into the little funnel-shaped gate gives this increased pressure if not relieved by an ample compensating outlet in the form of risers.

IRON TRADE REVIEW.

In the issue of October 28, E. H. Putnam contributes a paper on "The Melting of Iron in the Reverberatory Furnace and The Cupola." As the contents may prove of interest to many members of the Association, we reproduce the same.

If the young man wishing to learn iron founding so as to become a foreman could be sufficiently impressed with the importance of a thorough knowledge of the art of melting iron, his progress would be much more rapid and his success far surer. Many a first-class molder has utterly failed as foreman simply because he could not melt iron successfully. One of the chief hindrances to the progress of the novice is the marvelous melting done (on paper) by some modern foundrymen. The young man thinks he must melt 12 or 14 to 1 or "burst," and so he—bursts!

Don't do it, my young friend. Use enough fuel to insure hot iron at every tap, from first to last every day in the year; and, other things equal, your per cent of good castings, in excess of your competitor's who melts 14 of iron to 1 of coke will pay for all of the coke that both he and you use.

The melting of iron is indeed a great art, and the furnaces employed therein have been of many various designs; but probably the best of all is the reverberatory furnace. True, the cupola is usually preferred for ordinary castings, simply because it is the cheaper, and the castings produced therefrom, though not the best, are nevertheless acceptable. But, on the other hand, in certain classes of product, the matter of quality outweighs the question of cost; and then the reverberatory furnace is used. Sometimes both styles of furnaces are used in conjunction; as when the cupola will not hold sufficient iron for a casting. Then, the reverberatory furnace, heated by firing, as usual, becomes the storage receptacle of the molten iron tapped into it from the adjacent cupola. Here the iron is kept hot till enough has been accumulated for the casting required. Though the reverberatory furnace is employed in the melting and refining of all sorts of metals, yet its principal function is in the melting of cast malleable iron. Though the cupola is often used with advantage in this very important branch of iron founding, yet where great ductility and strength are required, the reverberatory furnace should always be used, as it unquestionably produces iron of a quality much better than that from the cupola. That is, it is more nearly perfect in homogeneity; it is much stronger, and it may be bent and twisted, after annealing, much more than cupola malleables would bear. This fact does not however utterly condemn the cupola as a melter for malleables, because there is a very large class of castings that are for all practical purposes quite as good for having a strength intermediate between that of gray iron and the strongest malleables. For this reason a great many manufacturers of cast malleables employ both kinds of furnaces. And some use two kinds of reverberatory furnaces—blast and natural draught furnaces. I am of opinion, however, that the natural draught furnace is not often seen, at least in the United States;

for Yankee push will not forego the use of an accessory that makes fair speed.

Reverberatory Furnace Construction.

There have been tried many modifications of shape, and of proportion in the construction of the furnace, in order to economical melting. The first desideratum is, of course, hot molten iron; then economy of fuel. To this end the sectional area is greatest at the hearth, and diminishes rapidly toward the stack. A few years ago all of the blast was introduced by way of the ash pit, through the gratebars. But this does not produce perfect combustion, a large amount of fuel in the shape of smoke escaping through the stack.

To remedy this, air is projected in from above at the bridge-wall, or furnace, end of the hearth. This portion of the blast enters through a number of small pipes, say, three inches or four inches diameter, whose longitudinal direction is such as to deflect the heat down on to the iron in the hearth. Thus, this added blast of atmospheric air produces more nearly perfect combustion, and tends to direct the heat to where it is most needed.

The bed, or bottom of the hearth, is composed of fire sand. The toughness of this course-sand bed, after firing over it for a couple of hours, is remarkable. The fire vitrifies the sand, but does not melt it. The hearth, therefore, becomes an unbroken and unbreakable floor upon which the heavy pigs of iron may be thrown without any danger of their breaking through.

The hearth, or portion of the furnace containing the iron, is lowest at the section where the tap hole is located, midway between the bridge-wall and the opposite end, the depth being governed by the disposition of the fire sand. The depth is determined by the quantity of iron that is to be melted. The hearth should be always as shallow as convenient for the amount of the "heat" or charge, so as to hold the iron well up against the action of the flame. The cope of this style of furnace is constructed of "bungs," so-called. A solid arched top would not answer because the heat is so intense that the bricks would frequently cave in, in various places, and then, in order to repair, the whole roof or

cope would have to be torn down. This would make simply an unbearable expense. The bung is a skeleton frame of cast iron filled with fire brick set on edge and held in place in the frame by means of a bolt which traverses the whole length of the bung and projects through a washer at each end. This washer is so adjusted as to press against the outer brick at each end of the bung, the nut being turned by a heavy, long wrench. The bricks are of the oblong shape, commonly called square. Their faces should be rubbed together till they fit closely; then they are dipped in a batter of fire clay, and set in place. There is but one row of brick to each bung, and the bungs are set side by side across the furnace until it is completely covered. After the furnace has been fired for two hours, more or less, a few of the bungs are removed from about the middle of the furnace, the charge of iron is thrown in and the bungs are then replaced. All cracks or apertures should be carefully stopped with mud. A few extra bungs should always be kept ready so that, if in the progress of a heat, any one or more of the bungs should give way, as indeed they sometimes do, it can be replaced without loss of time, or waste of heat. Just outside of the furnace, in the floor under the tap hole is a good sized pit partly filled with dry gang-way sand. This is a very important feature, and, though I have seen a furnace without it, yet it ought under no circumstances to be omitted. For the iron is apt to get away at times, and when there is no pit, it runs all over the floor, compelling the workmen to run away, burning the man who is trying to stop the tap hole and perhaps finally getting the advantage and taking its own extravagant way till the furnace is emptied. But with the pit, if the stream of iron gets a momentary advantage, still, as the iron falls down into the dry sand in the pit, it is at once covered from sight—there is no danger of its burning any one, and everybody knows it and so no harm is done. The walls of this pit are simply four cast iron plates set on edge. When the bull ladle is used the shank rests upon the end plates, which should rise somewhat above the floor.

Quality of Iron to be Used.

A different grade of iron is used in the reverberatory furnace from that which is chosen for the cupola. The iron which comes

from the cupola is, approximately, the same grade that went into it. But that which comes from the reverberatory furnace is of a distinctly different grade, sometimes widely different. Iron melted in the cupola is drawn from it practically as fast as it melts. But the iron that is melted in the reverberatory furnace remains under the action of the fire until all the charge is melted, and has become sufficiently hot and fluid for casting. This protracted firing burns away the silicon to a greater or less extent, thus rendering the carbon less apt to separate into the graphitic form in the casting. Of course this also takes place in the cupola, but to a very slight extent. So in making malleable iron from the cupola the mixture of iron put into it is, as to combined carbon, nearly what it will be after melting. In the reverberatory furnace a mixture is used whose proportion of combined carbon, is, before melting, very greatly less than it will inevitably be by the time it is sufficiently hot for casting.

In the making of malleable iron castings, the carbon should be combined with the iron to such an extent that no graphitic carbon may be seen in the fracture of the casting. That is, the carbon should be wholly in the combined state. And the mixture of irons for either style of furnace should be, to speak popularly, as soft as possible, consistent with this result. Any further firing or hardening is injurious, rendering the annealing more difficult.

Iron having the carbon in the combined state has a white fracture, is very hard, brittle and weak. Extract the carbon from this iron and it will be very soft, ductile and strong, and of solid structure. Iron having the carbon in the uncombined, or graphitic state, is soft and strong to a degree; that is, it is simply common gray iron. Extract the carbon from it and it will be a weak, cellular, worthless mass, because the graphitic carbon lies in interstices of the iron. Therefore, in order to benefit iron by extracting the carbon, the latter must be first chemically combined with the iron, when its removal will leave no pores or spaces between the particles, or atoms of iron.

Iron for Chilled Castings.

In making chilled rolls car wheels and other chilled castings, the desideratum is to have the carbon in the combined state in a

portion of the casting only. While it is true that malleable cast iron before annealing is almost, if not quite, as hard as chilled iron, yet the hardness is merely incidental, resulting from the, purposed, combination of the carbon. I may as well here state that chilling is also very frequently employed in malleable iron manufacture for the purpose of combining the carbon by rapid solidification in heavy portions of the casting; also in angles, where otherwise the shrinkage would cause a "crack" or separation of the iron crystals. For shrinkage is invariably from an angle in at least two directions, if no chill is used. But suddenly solidifying the iron at the angle by means of the chill causes it to contract at the time when it can draw freely from the surrounding fluid iron, and therefore no fracture results.

Combination is effected in malleable iron in order that the carbon may be extracted without leaving any porosity; thus producing a soft, strong iron; but in chilling the combination is effected with the express object of hardening. Fortunately, also, the chilling process causes the iron crystals to arrange themselves longitudinally perpendicular to the surface of the casting. This presents a surface of the utmost uniformity of texture and of the least possible porosity.

Iron comes from the smelting furnace in six or seven distinct grades, each grade varying in composition, so that any desired mixture may be formed by the educated manipulator. Uniformity of the mixture is of high importance generally, and in malleable and chilled castings especially. And this is emphatically true in the case of chilled rolls.

While either the cupola or the reverberatory furnace may be used in this work, yet undoubtedly the higher degree of perfection may be attained with the latter. I will not go so far as to say that this degree of perfection is always essential. Perfection is the costliest thing known to the mechanical world; and even an approach thereto is more than the average buyer is willing to pay for. In deciding as to the expediency of employing a mechanical process, the first necessary question is, will it pay?

To operate a reverberatory furnace economically there must be three heats a day; and it would take a pretty good sized business

in this specialty to permit of this. However, the art of melting iron in the cupola has advanced so rapidly of late years that the objections that formerly stood against this method have been largely stripped of their importance. The chemical changes necessary in the production of malleable cast iron render the reverberatory furnace almost indispensable in that branch of foundry. But these do not obtain in like degree in the manufacture of chilled rolls and of chilled castings generally. The skilled founder of chilled iron castings can be sure, therefore, of a product from the cupola of such uniformly excellent quality as to render him properly indifferent to the claims for the reverberatory furnace.

Cupola Management for Chilled Work.

It is not only possible, but it is easily practicable to produce from the cupola a uniform grade of iron day by day the year around. And this is the essential thing in making chilled castings. In the first place, the sand bottom in the cupola should be dry. But the question arises, What is meant by the word "dry." Well, it is rather difficult to define. It should not have sufficient moisture to cause the slightest danger of scabbing, or of making the molten iron boil. It should not be packed with a rammer, but should be trodden to moderate compactness with the feet. This will make possible the easy escape of the steam and gas through the sand and out of the cracks between the drop doors and the iron bottom. Then the fuel for kindling the fire should be dry. I have known wet kindling wood to change naturally soft gray iron to perfectly white iron. The walls of the cupola at the melting zone should present a fire brick surface and not a wet, mud surface. Sand stone spalls, or any sufficiently refractory material may be used for repairing, instead of fire brick, if desired. After "trimming out" the cupola, throw thick, heavy mud, preferably of fire clay, into the space to be repaired. Then carefully imbed the fire brick in such a manner as to present as little mud surface as possible.

The sum of area of tuyeres should exceed that of the blower outlet by about 50 per cent. This, in order to compensate for the greater frictional surface and for the partial obstruction by the

fuel. The coke used should be the best foundry coke and the bed should have sufficient depth to insure hot iron at the first tap.

The proportion of fuel to iron thereafter should be such as to insure hot iron at every tap—which is equivalent to stating that you must not try to melt 12 lbs. of iron to one of coke. For, though it is generally claimed that hard iron melts at a lower heat than soft iron, yet it cannot be claimed that it “stays melted” so long as soft iron. That is, a greater heat is required to make it fluid. Homogeneity is essential in chilled iron and extreme fluidity promotes this.

The size of the pieces of iron that enter the cupola should be proportioned to the size of the latter. This is far more important in chilled iron work than in ordinary casting. The mixture must be right, and if the pigs are long enough to cause any clogging of the cupola, a perfect mixture will be impossible. Let the fire burn through the bed of coke, and immediately charge the iron. Keep the cupola full to the charging door, till all the stock for the heat is on.

Provide a cupola ladle that will hold at least one charge of iron and tap into this till nearly full, before taking away the pour. This should give a pretty even mixture of iron. If you attempt to do without the cupola ladle and tap directly into the small pouring ladles, a portion of the iron may be too hard and another portion too soft. In charging the cupola, do not make the charges too large or the mixture may not be thorough.

The cupola should always be charged by a written formula and that formula should be kept, at least for a time, to refer to in case a change is desired. Every time the cupola ladle is filled a test casting should be made before the ladle is quite full, and this, cooled by plunging in cold water, should be broken, to ascertain if the chill is of the right depth. If too hard or too soft, tap into the ladle again before pouring and, if the mixture charged was right, your cast will also probably be right. By examining the test casts closely, any corrections desired may be made from day to day.

In conclusion, I will say that three or four different irons in a mixture, always including a liberal per cent of scrap—say

40 or 50 per cent—are much better than a less number of different brands. If there is a bad quality in one of the brands, not so much of it enters into the mixture, and the qualities of the other brands tend to neutralize it.

However, a good mixture may be made of two irons, and the current product of scrap and sprues provided the two irons be not too diverse in grade. Do not try to make chilled castings from all coke iron. But, having a good charcoal iron to rely upon for chilling tendency, modify that tendency by the use of some coke iron, if you choose.

THE TRADESMAN.

In writing of "Facings" E. H. Putnam says:

Charcoal is generally used in connection with a heavier or stickier facing. A heavy, refractory facing is first applied by shaking from a bag, and then the charcoal dust is applied in like manner. The heavy, sticky facing clings to the sand face of the mold with great tenacity, and the covering of charcoal dust prevents sticking to the tool while it is being smoothed down over the mold face. Charcoal dust is an excellent facing, and may easily be manufactured by anybody. If you cannot afford a mill to grind it in, why, just pound it up with a mortar and pestle. Then, if you have no heavy facing for a sticker, just shake a little flour on the mold; then apply the charcoal dust and slick it down. But the king of all facings is silver lead.

I speak of the facings that are applied after withdrawing the pattern. Silver lead is a mixture of plumbago, or graphite, with something else. Or, if you please, it is a heavy facing with a large per cent of graphite mixed with it. It is best applied by the bag, because of the facility with which it may thus be distributed. On all surfaces where convenient the smoothing should be done by the hand. Just, with the palm or open hand, rub it down all over the face of the mold. If at any spot the mold face roughs up a little apply some more silver lead, and rub down again. Sometimes, for special work, a camel's hair brush is used and, if you choose, you may use the slick and trowel where the hand can not be used to better advantage.

A good article of silver lead will not cut or wash if properly applied, and it is the easiest of all facings to apply.

It may be used in brass molding to advantage. Also in casting lead. In this latter case it leaves nothing to be desired. Every foundryman knows how easy it is to get a lead casting so rough that it is scarcely recognizable—how the lead “eats” into the sand, and in places the casting seems twice as thick as the patterns. All this trouble is entirely avoided by the use of silver lead.

Ram the mold somewhat harder than for iron—quite a good deal harder—and smooth the silver lead thoroughly everywhere, and the resultant cast will be true to pattern, and so smooth that it may in turn be used for a pattern without further dressing.

The same writer advances the following opinion on “Chemistry”:

Chemistry in the foundry has of late become quite a fad. I do not mean to deny chemistry any place at all in iron founding. But I do mean to say that it is not so universally important as is claimed for it. A large business in some lines of manufacture may no doubt employ a chemist with advantages. But what of the hundreds of foundries throughout the country where the additional expense of employing a chemist would absorb every cent of profit?

It is sometimes claimed that the chemist would so cheapen cost by using cheaper grades of iron as to justify his employment. This, too, may be true in some factories, and in certain classes of work. But, generally, it is not true. For scores of foundries are already using the cheapest possible mixture of iron and that without any assistance from the chemist.

Where a firm is manufacturing a special line of castings, requiring, presumably, very costly irons for the mixture, it might prove very profitable to employ a chemist. But it is probable that ere long the chemist will be employed just where he belongs, viz., at the smelting furnace, and that every carload of iron will be sold guaranteed as to chemical constituents.

It is fashionable just now to sneer at the founder who does not swallow whole everything that is said in favor of chemistry in the foundry. But I predict that before long the chemistry fad,

in so far as it applies to general founding, will be found to have gone with the center blast fad—and where is that?

Some years ago manufacturers of chilled iron castings used exclusively charcoal iron, and thought that they had to do so. But recently many have used mixtures of charcoal and coke irons, and have found them quite satisfactory. It may be that some founders did not learn to do this till advised by the chemist. There are many people in all lines of manufacture who never get out of the rut till they are jerked out. But there are certainly some founders who got out of the all-charcoal iron rut without any assistance from chemistry.

Perhaps after all the best test of the quality of a founder's product is the measure of success that his business enjoys. Judged by this criterion there does not seem to be any general demand for chemists in the operating force of iron foundries.

The chemist is sometimes needed, but the test bar is, generally, by far the better (because cheaper, and quite effective) means of guiding in the mixture of irons.

An eastern chemist wrote me some months ago, offering his services to enable me to make a mixture of chilling iron, stating what chemistry would enable him to do. As I had been doing the very thing that he proposed for a period of years with uniform success without the slightest aid from chemistry I did not need him. The physical test had been my only guide, and it was sufficient.